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ABSTRACT

The effects of adult education in welfare-to-work programs were examined in a national evaluation of welfare-to-work strategies. The evaluation used a random research design to estimate the overall effects of welfare-to-work programs in the following states: Georgia, Ohio, Michigan, Oklahoma, Oregon, and California. The study focused on the following issues: the quality of the education services provided; the extent to which welfare recipients participate in education and earn education credentials; the value of the education services provided; and the value of basic skills and education credentials in the labor market during the mid-1990s. The following were among the key findings: (1) even when welfare recipients preferred not to enter adult education, welfare-to-work programs substantially increased their receipt of such education; (2) assignment to education-focused programs did not generally appear to have substantial payoffs for welfare recipients in terms of education outcomes; and (3) earning a General Educational Development (GED) certificate, increasing basic skills, or subsequently participating in postsecondary programs yielded substantial benefits in terms of employment, earnings, and self-sufficiency. The following items are

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appended: discussions of the data sources and research samples; five supplementary tables; and descriptions and discussions of the survey instruments and subgroups.(Contains 63 references and 54 tables/figures.) (MN)

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National Evaluation of Welfare-to-Work Strategies

Improving Basic Skills: The Effects of Adult Education In Welfare-to-Work Programs

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National Evaluation of Welfare-to-Work Strategies

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Appreciation is also due the adults in the NEWWS Evaluation sample. Whether as participants in one of the welfare-to-work programs studied or as members of the control group, these men and women shared information about themselves, participated in surveys, and completed literacy and math tests. Without them, the research would not have been possible.

Three external reviewers also deserve acknowledgment. Edward Pauly, formerly of MDRC and now director of research at the Wallace–Reader's Digest Funds, originally conceived of this special study of adult education for welfare recipients. Having graciously remained involved in the project, he reviewed the entire report and provided helpful comments and suggestions. The report also benefited from the contributions of Richard Murnane and Ronald Ferguson, both professors at Harvard University, who reviewed some of the complex analytical procedures used in this work.

Chapter 1

Adult Education in Mandatory Welfare-to-Work Programs: Introduction, Synthesis, and Implications for Policy

I. Introduction

Since the early 1980s, welfare policymakers and program operators have debated the role of adult education in program strategies to help welfare recipients make the transition from welfare to work. The so-called Human Capital Development, or HCD, strategy focuses on providing education and increasing welfare recipients' basic academic skills and education credentials, following research evidence that these skills and credentials are prerequisites to obtaining stable employment.[1] This HCD strategy became popular especially during the mid to late 1980s. For example, the California Greater Avenues for Independence (GAIN) program, initiated in 1986 and evaluated by Manpower Demonstration Research Corporation (MDRC), provided education and training to large numbers of welfare recipients. During the same time, the federal government also placed a greater emphasis on adult education, as evidenced in the Family Support Act (FSA) of 1988, Public Law 100-485.

However, during the early 1990s, alternative program strategies gained popularity, seeking rapid job entry for welfare recipients instead of providing them with education first. Such strategies—known as Labor Force Attachment (LFA), or "work first," strategies—are supported by research findings suggesting that quickly entering the labor force is a promising trajectory to long-term self-sufficiency.[2] For example, in 1995, California ended testing of literacy and math skills as welfare recipients enter the state's welfare-to-work program, thereby eliminating these tests as a way to determine who initially needs education services. This turn toward more work-focused welfare-to-work programs was reinforced by the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, P. L. 104-193, which placed time limits on welfare receipt, making longer stays in education and training programs less attractive for program operators and welfare recipients.

However, the debate surrounding HCD programs for welfare recipients is not settled. Although LFA programs appear to be more effective than education-focused programs in the short term, there is no proof that offering job search programs to all welfare recipients (regardless of their education needs) leads to long-term self-sufficiency for a majority of the welfare caseload. A significant number of programs currently operated for welfare recipients offer education classes among their array of possible services, and such services are likely to remain important under Temporary Assistance for Needy Families (TANF), the funding structure for PRWORA.

Comparisons of HCD and LFA strategies are not a major focus of this report. Instead, this report mostly addresses key questions about *how* HCD programs in general, and adult education activities in particular, affect the educational and economic outcomes of welfare recipients. Specifically, these analyses move beyond overall HCD program effects and focus on participation in adult education and the effects of such participation. This is important, because many welfare recipients who are assigned to HCD programs do not enroll in adult education classes or they drop out of these classes after only a brief spell of participation. As a result, the impacts of the larger programs do not necessarily reflect the full potential of the adult education services provided to those who receive them. Learning more about how those specific services affect participants is the primary focus of this report.

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[1] Mincer, 1974; Polachek and Siebert, 1993; Sum et al., 1995.

[2] See, for example, Riccio et al., 1994.

Background Information: Research Design for the National Evaluation of Welfare-to-Work Strategies

This report is part of the National Evaluation of Welfare-to-Work Strategies (NEWWS)—an evaluation of programs begun under the Family Support Act, conducted by the Manpower Demonstration Research Corporation, and funded by the U.S. Department of Health and Human Services, with support from the U.S. Department of Education. The evaluation includes programs in seven sites across the country: Atlanta, Georgia; Columbus, Ohio; Detroit, Michigan; Grand Rapids, Michigan; Oklahoma City, Oklahoma; Portland, Oregon; and Riverside, California. It uses a random assignment research design to estimate the overall effects of the welfare-to-work programs being studied.

In each site, individuals who were required to participate in the program were assigned, by chance, either to a program group that had access to education, training, and other employment services and whose members were required to participate in the program or risk a reduction in their monthly welfare grant (a "sanction"), or to a control group, which received no services through the program but whose members could seek out services from the community. This random assignment design ensures that there were no systematic differences between the background characteristics of people in the program group and those in the control group when they entered the study. Thus, any subsequent differences in outcomes between the groups can be attributed to the program.

It is important to note that the programs in the evaluation were not subject to the conditions and requirements of what are currently referred to as "welfare-to-work" programs as defined under TANF. During the follow-up period analyzed in the report, individuals in the 11 studied programs did not face a time limit on eligibility for welfare assistance as they would now under TANF. All the programs, however, shared TANF's primary goal of moving welfare recipients into paid work and off assistance. Furthermore, among the 11 programs are some which are strongly employment-focused—the welfare-to-work strategy favored under TANF—as well as some which are strongly basic education-focused. The programs varied in many other ways as well, including how broadly the participation mandate was applied to the welfare caseload and how strictly it was enforced, the amount of child care support provided for program participation or employment, and methods of case management. The programs also served different welfare populations and operated in a variety of labor markets. Lastly, it is important to point out that the programs being evaluated here are state-operated welfare-to-work programs originally developed under the Family Support Act of 1988. These programs are unrelated to the "welfare-to-work" programs currently being operated by local Workforce Investment Boards, supported by the Workforce Investment Act, and administered by the U.S. Department of Labor.

The research design for the study presented in this report often diverges from that of the larger NEWWS study. Instead of comparing randomly assigned program group members and control group members, this study often compared the experiences of welfare recipients who participated in certain adult education activities with those of recipients who did not and sought to estimate how varying degrees of participation affected education and employment outcomes. A detailed discussion of these "nonexperimental" comparisons is featured in a separate text box on page 7.

A. Purpose of This Study

The analyses presented here help answer many important policy questions surrounding adult education for welfare recipients. These questions concern (1) the quality of the education services provided, (2) the extent to which welfare recipients participate in education, (3) the extent to which welfare recipients earn education credentials, (4) the value of the education services provided, and (5) the value of basic skills and education credentials in the labor market during the mid-1990s. These issues are addressed primarily by comparing the experiences of recipients who participate in adult education with those of recipients who do not and by assessing the relative effectiveness of different *levels* of participation in adult education.

B. Overview of This Report

These analyses of how adult education works in the context of welfare-to-work programs were conducted for a large sample of welfare recipients who entered one of the 11 programs studied in the National Evaluation of Welfare-to-Work Strategies (NEWWS) *without* a high school diploma or General Educational Development (GED) certificate.[3] Thus, the findings do not generalize to welfare recipients who *do* have a high school diploma but who still may be served by HCD programs that provide more advanced levels of education and training. The types of adult education examined in the report encompass adult basic education (ABE) classes, programs preparing students for the GED exam, regular high school classes, and classes in English as a Second Language (ESL). Among these, ABE and GED preparation accounted for most of the adult education in the 11 mandatory welfare-to-work programs studied. These 11 programs operated in seven sites, and each program was operated under the federal FSA and its Job Opportunities and Basic Skills Training (JOBS) program. (Program intake for this study began in June 1991 and ended in December 1994; data presented cover June 1991 through December 1997. See the accompanying box for further information about this study and report.)

This chapter summarizes most, but not all,[4] of the analyses presented as a collection of papers in this report, specifically addressing the following questions:

- What are the characteristics of adult education providers in welfare-to-work programs? What are typical attendance patterns in these classes?
- To what extent, and for whom, do welfare-to-work programs increase participation in adult education services and increase educational attainment and achievement?
- Do education-focused welfare-to-work programs improve education outcomes?
- What is the payoff to additional participation in adult education?

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[3] GED refers to the GED credential and the exams that individuals must pass to attain it. In this report, we use GED to refer to both the credential and the exams.

[4] For example, Chapter 5 of this report presents comparisons of program impacts on earnings and welfare receipt for welfare recipients in LFA and HCD programs. Because these comparisons do not directly relate to participation in adult education and outcomes directly associated with such participation, these analyses are not summarized here.

- How do education outcomes and milestones affect the employment outcomes and self-sufficiency of welfare recipients?
- Among those who participate in adult education, who moves on from adult education to receive postsecondary education and training, and how does this contribute to their earnings and self-sufficiency?

The analyses in the report take a unique perspective on adult education, one that will be of interest to the adult education community as well as to those involved in welfare policy. First, our analyses reflect on the effectiveness of adult education services provided to a highly disadvantaged group of students: low-income, mostly jobless, single-parent women who lack a high school diploma or GED and are receiving welfare. This group represents a significant share of all adult education students. One study found that, in 1992, 22 percent of all new students in U.S. adult basic education, high school completion, and GED programs had received public assistance in the year before enrollment; about 11 percent of all new ESL students met this criterion.[5] This same disadvantaged group is likely to be of increasing concern to welfare policymakers. Drastic reductions in welfare caseloads since their peak in 1994 are also changing the face of caseloads, which now increasingly consist of "hard-to-serve" recipients. It is likely that many of those left on the welfare rolls will lack an education credential and will have poor reading or math skills.[6]

Second, the adult education programs studied and their effects on students reflect the fact that these programs operate within the context of welfare-to-work programs. Such programs provide other services, such as counseling, child care, job search assistance, and postsecondary education and training. Although many issues facing adult educators are essentially unrelated to the welfare status of the students they serve, the context of available supports, expectations, and requirements is different for those enrolled in adult education as part of a welfare-to-work program. Thus, the measured effects of these programs reflect not only the payoff to adult education but the effects of a larger package of services and requirements that included adult education. As part of such a package, the adult education provided could be strengthened to produce greater effects (for example, if students receive help with child care or transportation). However, the effects of adult education also could be weakened by other program components (for example, if program rules limit the time that students may be enrolled in adult education or if the program emphasizes a quick transition from welfare to work).

Third, for welfare recipients in our study, participation in adult education was mandatory. While "traditional" adult education students enroll on a voluntary basis and can therefore be presumed to be motivated to learn, such motivation may sometimes be lacking when students are compelled to participate by mandatory welfare-to-work programs. Like most other adult education students, those mandated to participate often have done poorly in school in the past and may be alienated from traditional educational institutions and modes of instruction. Unlike the voluntary, or traditional, students, however, students connected to a welfare-to-work program may initially be motivated to attend classes less by the desire to learn or to obtain a credential than by the

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[5] Young et al., 1994, p. 15.
[6] Danziger et al., 1999.

Definition of Key Terms

Many of the terms used in this report are not reintroduced and redefined in each chapter. In some cases, these terms are ambiguous or have a somewhat different meaning in the context of welfare-to-work programs than is common in the adult education field. In this box, we introduce some of the most commonly used terms in this report.

Throughout the report, *adult education* refers to any or all of the following:

- **Adult basic education (ABE) classes.** These provide reading and math instruction to students whose achievement levels are lower than is required for high school completion or GED classes—typically at the eighth grade level or lower.
- **General Educational Development (GED) classes.** These prepare students to take the GED tests that are used by states to award certificates signifying knowledge of basic high school subjects (social studies, literature, science, math, and writing). Students entering GED classes usually are expected to have language and math skills at a ninth grade level or higher so that they can use the instructional materials.
- **High school completion classes.** These replicate a high school curriculum in an adult school setting. Students usually must have language and math skills at a ninth grade level or higher to enter a high school completion program. When students finish the course of study, they receive a high school diploma.
- **English as a Second Language (ESL) classes.** These provide instruction in how to read and write English to people who are not fluent English speakers.

The term *welfare* encompasses both Temporary Assistance for Needy Families (TANF) and its predecessor, Aid to Families with Dependent Children (AFDC).

Program group members are sample members who were randomly assigned to a welfare-to-work program.

Control group members are sample members who were randomly assigned to a control group, were excused from a welfare-to-work program mandate, and were ineligible to receive most program services. However, control group members could seek out similar services in the community on their own.

Program participants are sample members who participated in a particular program activity. This could be a class, a training program, a job club, or something similar. Program participation is not limited to program group members assigned to welfare-to-work programs, because control group members could access services outside welfare-to-work programs on their own. Depending on the context, program participants are sometimes referred to as "students," as "enrollees," or as "recipients" of education or training services.

need to comply with welfare-to-work program requirements in order to avoid reductions in their welfare grant.

Finally, in the context of welfare-to-work programs, adult education is viewed as an intermediate goal, not as an end in itself. Financial self-sufficiency of adult students and their families is the ultimate goal of these programs.

These factors have helped shape the analyses and interpretation of results in this report. However, one additional factor—one of importance to the adult education community—cannot be taken into account in this examination. The prevalence of learning disabilities among welfare recipients is estimated to be between 25 and 50 percent.[7] As implemented during the study period and probably continuing into current operations, most programs did not assess welfare recipients for learning disabilities, which could affect the programs' ability to address these disabilities, clients' skill development in the programs, and clients' subsequent labor market success.

C. Findings in Brief

Although the five chapters following this one use various analytical techniques and samples, taken as a whole they support the following broad conclusions about adult education in the NEWWS welfare-to-work programs serving those without a high school diploma or GED:

- In providing services for welfare recipients, adult education programs generally did not adapt their curricula or teaching methods to fit the specific needs of this group of students.

- Even when welfare recipients preferred not to enter adult education, welfare-to-work programs substantially increased their receipt of such education. There was no evidence that those who were mandated to participate (most of whom did not express a preference for adult education) benefited any less from their participation in terms of educational attainment and literacy or math gains than those who volunteered.

- On the whole, assignment to education-focused programs did not appear to have a substantial payoff for the welfare recipients in our study in terms of their education outcomes. Although the programs increased GED receipt, most participants did not earn a GED, and few experienced significant increases in their reading and math skills. Three-year impacts on earnings and welfare receipt in HCD programs were smaller than those experienced by welfare recipients in LFA programs.

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[7] See, for example, Center for Law and Social Policy, 1998, and Pavetti, 1997.

Combining Experimental and Nonexperimental Research Methods

The random assignment research design (described in the box on page 2) was used for certain analyses in this report, but others were conducted with *nonexperimental* methods, which go beyond the random assignment research design. In general, the distinction between these two methodologies depends on whether a question concerns effects of the programs *as a whole*, which the random assignment research design is well equipped to address, or whether a question concerns the effects of program *components* or program *outcomes*, such as participation in adult education, GED receipt, or participation in postsecondary services. Because of the protection offered by the random assignment research design, findings about the programs as a whole (concerning questions like "By how much did the HCD programs increase participation in adult education?") are more reliable and can be presented with greater confidence. Findings about program components or outcomes (concerning questions like "By how much does a GED increase subsequent enrollment in postsecondary education or training?") are not protected by random assignment and therefore have greater uncertainty surrounding them.

In addition to the difference in research methods, the two types of questions outlined here differ in their substantive focus. The "program" questions are less precise than the "component" or "outcome" questions, describing how assignment to a broad program affected outcomes, not how specific events and services did. For example, many of those assigned to welfare-to-work programs did not participate in program activities, or participated for short periods. This limits the extent to which the program could affect sample members' education outcomes. No such limitations exist for analyses involving specific program components or outcomes because participation is explicit in the definition of the measure studied. An estimate of the effects of GED receipt on earnings in principle (and on average) applies fully to everyone who received such a credential, as does an estimate of the payoff to an additional month of adult education. Thus, in summary, the experimental findings presented are robust and reliable but apply to programs that do not always reach participants as intended, whereas the nonexperimental findings are less reliable analytically, but answer more concrete policy questions facing the adult education community.

- Gains in reading skills appeared to vary with the length of time spent in the adult education programs. Stays shorter than a year (which the majority of participants in adult education had) did not improve reading skills measurably, whereas longer stays were associated with substantial gains, comparable—for this sample—to those associated with regular high school attendance.

- Improvements in math skills were associated with shorter spells of adult education. After six months of adult education, most participants' math skills no longer improved.

- GED receipt also was associated with shorter spells of participation in adult education. Additional participation beyond six months did not increase GED receipt, possibly because most GED recipients were close to being able to pass a GED test when they entered the programs.

- Higher average levels of teachers' experience and education in the adult education programs appeared to enhance the payoff to participation in adult education in terms of reading and math skills.
- The welfare recipients who were most likely to get GED certificates and receive postsecondary services were those who had higher initial reading and math skills when they entered the welfare-to-work programs.
- As students earned GEDs, increased basic skills, or subsequently participated in postsecondary programs, they appeared to have substantial benefits in terms of employment, earnings, and self-sufficiency. However, relatively few adult education participants received a GED, increased their basic skills, or entered postsecondary programs.
- Receipt of a GED credential was an important predictor of subsequent enrollment in postsecondary programs. Participants in basic education programs who went on to postsecondary education or training programs appeared to experience substantial benefits from them in terms of increased earnings and self-sufficiency.

II. Summary of Each Chapter's Findings

This report on adult education in 11 welfare-to-work programs addresses many different aspects of the adult education experience of welfare recipients in welfare-to-work programs. Specifically, the report traces the steps outlined in Figure 1.1,[8] which describes the hypothesized effects of adult education in welfare-to-work programs.

In Chapter 2 we focus on the second and third boxes in Figure 1.1, addressing the questions of how education-focused welfare-to-work programs affect participation in adult education, what adult education that is provided to welfare recipients looks like, and how these welfare-to-work programs affect education outcomes. Specifically, program effects are presented on participation in adult education, basic skills, and GED receipt in three education-focused programs (in Atlanta, Grand Rapids, and Riverside). We explore how these effects vary across different subgroups and attempt conceptually to link effects on one outcome to effects on other outcomes.

In Chapter 3 this analysis is taken a step further, focusing more directly on the third box in Figure 1.1, describing the relationship between participation in adult education and improvements in the skill and educational attainment of participants. For example, we examine how an extra month of participation affects key education outcomes and how this effect varies with (1) total time spent in adult education, (2) individual student characteristics, and (3) program and staff characteristics.

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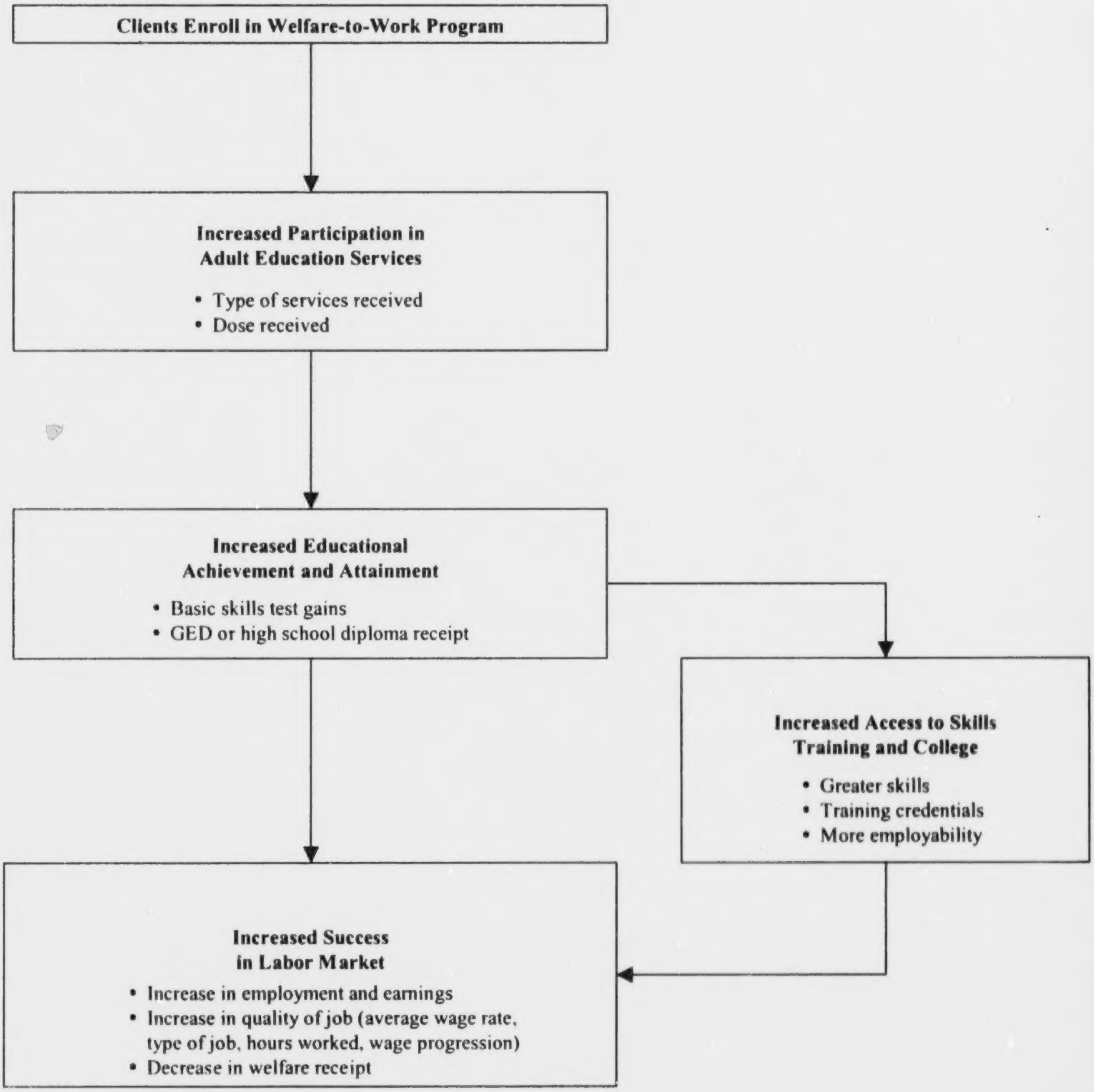
[8] Adapted from Martinson and Friedlander, 1994.

Figure 1.1

For Sample Members Without a High School Diploma or GED at Random Assignment: Hypothesized Effects of Adult Education in Welfare-to-Work Programs


Clients Enroll in Welfare-to-Work Program
Increased Participation in Adult Education Services
• Type of services received
• Dose received
Increased Educational Achievement and Attainment
• Basic skills test gains
• GED or high school diploma receipt
Increased Access to Skills Training and College
• Greater skills
• Training credentials
• More employability
Increased Success in Labor Market
• Increase in employment and earnings
• Increase in quality of job (average wage rate, type of job, hours worked, wage progression)
• Decrease in welfare receipt

In Chapter 4 we look at the fourth box and link education outcomes to employment outcomes and self-sufficiency, addressing questions like "What is the GED worth to welfare recipients?" and "How are additional reading and math skills valued in the labor market?" Chapter 4 uses survey and administrative data across all 11 welfare-to-work programs in each of the seven NEWWS sites.

Chapter 5 revisits the experimental comparisons (of randomly created program and control groups), comparing all 11 programs for their effects on earnings and welfare receipt and attempting to isolate factors that made some programs more successful than others.

Last, Chapter 6 completes our analyses by focusing on the box to the right in Figure 1.1, addressing the important intermediate step of postsecondary education and training, which is often believed to be an important intermediary link between participation in adult education and longer-term improvements in earnings and other employment outcomes. The chapter addresses the questions: "What determines whether adult education participants enter postsecondary programs?" and "Do participants benefit from these programs?"

A. Characteristics of Adult Education in Welfare-to-Work Programs and the Effects of Education-Focused Welfare-to-Work Programs on Educational Attainment and Achievement (Chapter 2)

Site visits and surveys of education providers in the HCD programs in Atlanta, Grand Rapids, and Riverside found that these welfare-to-work programs used a wide variety of educational institutions to provide adult education to the welfare recipients enrolled in these programs. Research conducted at the adult education provider sites concluded that the inclusion of welfare-to-work program participants in the adult education classes usually did not greatly affect the providers' operations, curricula, and teaching methods. In other words, welfare-to-work program participants took classes together with non-welfare recipient adult education students and generally did not receive services specially tailored to their needs from adult education providers or classroom teachers. In some cases, hours were expanded to enable welfare recipients to participate for 20 hours a week, as required by welfare-to-work program regulations. In other cases, additional counseling or job-readiness instruction were added for welfare recipients. Only in Riverside did the welfare-to-work program negotiate contracts with adult education providers and use welfare-to-work funds to pay providers serving their clients. However, aside from these contractual differences, there were few systematic differences in the adult education provided across the three sites.

The welfare-to-work programs substantially increased participation in adult education for those who entered the study without a high school diploma or GED (the sample analyzed throughout the report). Without the programs, about one-fifth of sample members sought out adult education programs on their own (as evidenced by two-year participation rates in the control group). The three programs studied in Chapter 2 (Atlanta, Grand Rapids, and Riverside) more than doubled this rate of participation: one-half of program group members participated in adult education. When they enrolled in adult education, program group members also stayed longer. Across all sample members (including those who did not participate at all), the program more than tripled the average number of hours of adult education, from 68 for control group members to 244 for program group members. This means that the average *participant* in program-provided adult education classes was enrolled for about 488 hours. Thus, the programs induced more individuals to participate in adult education, and those who participated did so for more hours.

As part of the analysis, impacts on adult education participation were estimated for 20 different subgroups, defined using individual characteristics measured at program entry (hereafter referred to as "baseline characteristics").[9] Examples of such subgroups include persons with young children, those who dropped out of school having completed eighth grade or less, those expressing a lack of desire to go back to school, and those with personal or family barriers to participation. Without exception, the three HCD programs increased participation in adult education for each of these subgroups. This shows that mandatory welfare-to-work programs can increase welfare recipients' exposure to adult education even among welfare recipients with barriers to participation.

The three programs achieved modest impacts on GED receipt during a two-year follow-up period. Whereas only 4 percent of control group members received a high school diploma or GED during the follow-up period, 11 percent of program group members received such a credential. This impact more than doubled the proportion with an education credential; however, fewer than one in five participants in adult education earned a credential. (Many sample members might not be expected to attain such a credential during the two-year follow-up period, because they entered the programs with low achievement levels or limited English skills.)

Moreover, the three programs did not increase scores on standardized reading and math tests, conducted as part of the two-year follow-up interview. As discussed more extensively in Chapter 2, the combination of modest increases in GED receipt and a lack of gains in measured literacy and math skills has been found in several previous studies. There are several possible explanations for this apparent discrepancy. First, it is possible that the GED test and the basic literacy and math tests administered in the survey do not capture the same underlying skills. In that case, someone might be able to pass a GED test without showing concomitant gains in basic reading and math skills. It also is possible that difficulties with the administration of the literacy and math tests reduced the statistical reliability of our findings. (These tests were administered as part of a long interview in sample members' homes—not the ideal environment in which to concentrate on a skills test.) However, it is reasonable to conclude that, on average, participants experienced limited benefits in terms of increased skills and credentials from their participation in adult education. Subsequent analyses presented in Chapter 3 and 4 further explore this issue.

Impacts on GED receipt and educational achievement also were estimated separately for 20 subgroups. Researchers found that impacts on GED receipt were strongest for those entering the welfare-to-work programs with already high reading and math skill levels. Those entering with high reading scores experienced an impact (that is, an increase relative to the control group) of 16 percentage points. Conversely, those entering with low reading scores experienced an impact of only 3 percentage points. Those entering the program having left school below the ninth grade experienced no impact on GED receipt. Thus, it seems that the programs' effects on GED receipt were closely tied to program participants' entry-level skills. Those who needed little basic education to earn a GED were much more likely to be successful in this regard. This finding is also consistent with prior research, involving both mandatory and voluntary programs for school dropouts.[10]

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[9] When subgroups are defined using preprogram baseline characteristics, comparisons of outcomes for program and control groups *within* these subgroups are considered fully "experimental"; that is, these comparisons constitute unbiased estimates of the program effects for those particular subgroups.

Interestingly, motivation to participate in adult education programs did not affect program success in terms of GED receipt. Even though some sample members indicated that they did not want to attend school, those who made this claim as they entered the programs and were required to participate anyway experienced substantial increases in GED receipt, just like sample members who did express a preference for adult education at program entry. This shows not only that welfare-to-work programs can induce individuals to do things they might not otherwise do but also that participants in adult education can be successful even if they prefer not to participate.

B. Individual Efforts and School Effects: The Payoff to Participation in Adult Education (Chapter 3)

Nonexperimental analyses in the three HCD programs discussed above (Atlanta, Grand Rapids, and Riverside) suggest that the amount of time that participants spent in adult education classes affected their educational attainment and achievement outcomes. However, these relationships were not straightforward. During participants' first year of participation in basic education, additional months of participation were not associated with higher literacy test scores. However, after a year of participation, additional months in adult education appeared to substantially increase test scores (an increase of .55 of a standard deviation for six additional months of participation).[11] In this sample, this effect was comparable to the differences in baseline literacy scores associated with having attended an additional month of high school. These findings suggest that a threshold level of participation of approximately one year is needed to achieve meaningful literacy gains lasting until the test administered in the two-year follow-up survey. (There could be a significant lag between the end of participation and this survey, which could make it difficult to reliably identify more modest gains in basic skills.)

For measured math skills, this relationship looked markedly different. Increases in math skills were associated with additional months of basic education during the first six months only. After that, no further increases in these skills were found. This suggests a "plateau" rather than a "threshold" type of relationship. Such a pattern could reflect limitations in the math skills being taught in adult education classes.

The relationship between time in adult education and GED receipt followed a similar pattern. Additional months of participation increased the likelihood of GED receipt during the first six months of participation but not thereafter.

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[10] See, for example, Quint et al., 1997, and Martinson and Friedlander, 1994.

[11] A standard deviation is a statistic capturing the variability of a particular measure or outcome in a sample. Effects on test statistics and other education outcomes are often expressed as a percentage of a standard deviation, because absolute changes in these outcomes are difficult to interpret.

When these analyses were conducted separately for program group members and control group members, it appeared that both groups experienced similar patterns of gains in literacy and math skills. This is interesting, because participation for program group members was mandatory, while control group members sought out adult education services on their own initiative and participated in them voluntarily. One might expect the payoff to the control group to be greater, but no such difference was found. A difference was found for GED receipt, but it was the opposite: program group members were more likely than controls to receive a GED as a result of additional participation in adult education.

When these analyses were conducted for different groups of sample members defined using baseline characteristics, little variation in the estimated effects of additional education on literacy and math skills was found. Sample members who faced greater barriers to participation or who were less motivated to participate in adult education benefited from additional education in similar ways as sample members who did not face these barriers. However, the analyses also found that the relationship between adult education participation and GED receipt varied across the subgroups. The increased probability of earning a GED from short-term participation, as mentioned above, did not hold up for sample members entering the program having completed less than ninth grade. Conversely, it was found that, for those with low initial skill levels, the likelihood of GED receipt continued to increase with additional participation beyond six months. This is unsurprising, because one might expect longer-term participation in adult education to help those who started out with low skills more.

Last, we analyzed how the relationship between time spent in adult education and education outcomes was affected by differences in provider characteristics, including measures such as class size, teachers' experience and education levels, individual attention, the strength of the link between the education provider and the welfare-to-work program, and program exit standards. The reliability of this analysis was limited, because we were unable to study the links between individual teachers and individual students, instead having to correlate student outcomes with school-level averages of teacher characteristics. However, within these limitations, teachers' experience and teachers' education each appeared to enhance significantly the payoff to additional time spent in education classes in terms of reading and math skills. No provider characteristics were identified that affected programs' effectiveness in increasing GED receipt. (However, provider data were limited, and the same caveats about our ability to match students to specific teachers applies here.)

C. Does the Low-Wage Labor Market Value Basic Education? Effects of GED Receipt and Literacy Gains on the Self-Sufficiency of Welfare Recipients (Chapter 4)

Having analyzed how participation in adult education programs affects literacy, math scores, and GED receipt, the next step (in Chapter 4) is to analyze the extent to which improvements in these education outcomes affect welfare recipients' employment outcomes and welfare receipt. If those effects are strong, improving welfare-to-work programs' ability to improve education outcomes would also improve the programs' effectiveness in terms of employment and welfare receipt. However, this would not be the case if these education outcomes had little effect on sample members' employment and welfare receipt. In that case, a focus on immediate employment might be more appropriate.

The analyses presented in Chapter 4 also contribute to our knowledge about the value of the GED and the importance of basic skills in the low-wage labor market. Much of the GED-related research has focused on comparisons of GED holders and high school graduates, using national data sets. Those findings may have limited relevance for programmatic choices made on behalf of welfare recipients, whose needs and experiences may be different from those of other school dropouts.

GED receipt appeared to substantially increase earnings. The estimated effect on annual earnings in the third year of follow-up was approximately $771. GED recipients also received fewer welfare benefits (an estimated reduction of $331). These estimated effects remained largely unchanged when measures of time spent in adult education or of reading test scores were introduced as control variables into the analysis. This suggests that our estimates of the value of this credential reflect the effects of the credential itself, not the underlying basic skills or participation in adult education programs. It also was found that earning a GED had stronger estimated effects for program group members than for control group members. This suggests that the other aspects of the welfare-to-work programs (ranging from the program's message to services like job search assistance, skills training, and college programs) further enhanced the value of this credential by increasing participants' ability to make use of their newly acquired credential in the workplace.

Like all analyses of educational attainment, analyses of GED receipt are potentially affected by selection bias. Such bias occurs when recipients of GED credentials are different from nonrecipients in ways that are not controlled in the analysis. In Chapter 4, effects of GED receipt were estimated in various ways to assess the sensitivity of the findings to selection bias and other problems. In general, the different estimates were consistent with one another, and there was no evidence that uncontrolled differences in motivation or ability explained the apparent effects of GED receipt on earnings and self-sufficiency. However, the findings presented in Chapter 4 could not be confirmed with an advanced statistical method (an "instrumental variables" estimator) because statistical precision was lacking.

Analyses of the effects of greater reading skills on employment outcomes and self-sufficiency found those effects to be substantial. An increase of one standard deviation in reading scores was associated with $355 in additional earnings during the year following the test (the third year of follow-up). This effect was independent of (that is, in addition to) any effect from earning a GED credential. (Introducing math skills separately did not show an independent effect separate from that associated with greater reading skills.) All this suggests that HCD programs could have more substantial effects on the economic outcomes of welfare recipients if these programs managed to improve their effects on mediating education outcomes. Our analyses suggest that increased retention might be one way to achieve this. Research on "best practices" in adult education for welfare recipients has suggested that programs' ability to retain students and improve their skills is affected by many program characteristics, including:[12]

- developing a well-defined mission,
- providing specially targeted classes to students who are welfare recipients,

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[12] Adapted from Quint, 1997, p. 10.

- having skilled, experienced teachers,
- emphasizing staff development,
- adopting varied instructional approaches, including small group and computer activities,
- communicating frequently with welfare-to-work program staff,
- stressing regular attendance,
- aggressively following-up on absences,
- adopting relatively intensive class schedules, and
- promoting a high degree of teacher-student and student-student interaction.

Finally, a greater emphasis on identifying and addressing learning disabilities, which now remain largely undiagnosed, could greatly improve programs' ability to serve their students successfully.

D. Beyond Basic Education: The Benefits of Skills Training and College (Chapter 6)

The last chapter of this report describes the effects of postsecondary education and training, focusing on participants in adult education programs across all 11 programs included in the NEWWS study. The chapter analyzes who among these participants go on to postsecondary education and training and how such postsecondary participation affects their employment outcomes and self-sufficiency.

The chapter reports that relatively few of those who participated in adult education programs (15 percent of participants) made it to postsecondary services during the two-year follow-up period. Among adult education participants, those who earned a GED and did so in a relatively short amount of time (less than a year) were most likely to enter postsecondary programs. These sample members also were more likely to have entered the NEWWS study with higher initial literacy skills and having completed more grades in high school.

Once enrolled in skills training or college programs, postsecondary participants remained in these programs for about seven months on average. More than 40 percent participated longer than six months, and almost 15 percent were enrolled longer than a year. At the time of follow-up, 29 percent of postsecondary participants were still enrolled in these programs, which means that these participation figures are preliminary and that participation could turn out to be longer on average when additional follow-up survey data become available. For those entering education programs without a high school credential, participation in postsecondary programs often follows a spell of adult education, suggesting that successful HCD programs for welfare recipients without high school credentials may require more time than many states' welfare time limits allow.

In nonexperimental analyses, participation in postsecondary programs was found to have substantial benefits in terms of greater earnings and lower welfare receipt. These benefits did not appear until after sample members completed their education and training. Effects for postsecondary participants appeared in the third year following their initial adult education spell. In that year, their earnings were $1,542 (or 47 percent) higher than those of sample members who received only adult education, while their welfare benefits were $919 (or 32 percent) lower. These estimated effects were not contingent on participants completing their spell of postsecondary education or training with a credential or certificate.

III. Conclusions and Implications for Policy

A. The Challenge of Making Adult Education Work for Welfare Recipients

This study of adult education for welfare recipients who do not have a high school credential uncovered several different patterns of effects. Assignment to a Human Capital Development program had substantial impacts on these welfare recipients' participation in adult education, modest impacts on their GED receipt, and no impacts on measured literacy and math skills. The study also found that, within a three-year follow-up period, the effects of HCD programs on earnings and welfare receipt were positive but limited, especially compared with labor force attachment programs.

A second pattern of findings concerns the dynamics of participation, learning, graduation, and skills acquisition that underlie the experimental impact story. The exploration of this pattern begins (in Chapter 3) with a discussion of one of the key questions underlying education and learning: What is the value of additional instruction? In addressing this question for three education-focused programs in Atlanta, Grand Rapids, and Riverside, Chapter 3 uncovers that additional months in school (our only reliable measure of additional instruction) matter; they increase literacy, math skills, and GED receipt. However, long-term participation (longer than a year) is necessary to achieve a measurable payoff in increased literacy skills, while increases in math skills and GED receipt seem to be limited to the first six months of instruction.

At the same time, the average program group member in these three programs received only about 244 hours (or about twelve 20-hour weeks) of adult education, which is substantially less than one year of high school—insufficient to make up the education deficit with which most of these welfare recipients entered the programs. This could explain why only 11 percent of program group members earned a GED during the follow-up period and why fewer than 15 percent of participants in adult education went on to skills training and college programs.

Next, using nonexperimental methods, the report shows that for those who did reach these milestones, participating in adult education was beneficial. Chapters 4 and 6 show that payoffs from GED receipt, increased literacy skills, and postsecondary education and training were substantial. Thus, in summary, the analyses presented in this report confirm the internal logic underlying the human capital program model (as outlined earlier in Figure 1.1). However, too few program group members made it through the different steps to experience the anticipated payoffs at the end. Assuming that other program participants would experience comparable benefits, the challenge facing welfare-to-work program administrators and adult education providers is to find a way to retain more students long enough, to help them reach the immediate goal of earning a GED, and to help them access postsecondary services that allow them to capitalize on this credential. This could occur while people are receiving welfare benefits or after they have left the welfare rolls. However, there is no guarantee that simply increasing the duration of participation is sufficient to reach educational goals. The quality of instruction, the appropriateness of the material and technology for participants with low skills and possibly with learning disabilities, and the sometimes limited motivation of program participants are likely important as well, although our study did not examine these factors.

B. Policy Implications

- **Education-focused interventions for welfare recipients with low basic skills can improve these skills and increase GED receipt. However, improvements in these outcomes may require long spells of participation in adult education programs.**

The analyses presented in this report show that the long-term payoffs of an education-focused approach for welfare recipients without a high school diploma or GED can be substantial. However, the report also shows that it is a challenge to improve the basic skills and educational attainment of welfare recipients, even in programs that are directly focused on education outcomes. For those entering with low skills and lacking years of high school, several years of basic education and postsecondary education may be needed to promote long-term success and self-sufficiency. In the current welfare environment, such a long-term commitment carries some risk inasmuch as long-term participation may exhaust welfare recipients' limited allotted time on welfare.

- **For welfare recipients who are within easy reach of earning a GED, pursuit of such a credential is a good program option that produces substantial benefits, increasing welfare recipients' earnings and their access to postsecondary education or training.**

Success is easier to achieve for those who are close to passing the GED test. Our findings suggest that this credential is a worthwhile short-term program goal, especially if it is combined with a targeted postsecondary activity. Together, a GED and skills training greatly increased earnings and reduced welfare receipt in the third year of follow-up. Education-focused welfare-to-work programs may be most successful when they can combine GED preparation and postsecondary services in a relatively short and intensive program (an option that, however, will not work with the most educationally disadvantaged).

- **Too few adult education students and GED recipients continue on to postsecondary education or training. Links between adult education programs and postsecondary programs could be strengthened, and adult education students should be made aware of the limitations of having just a GED credential as a way to improve one's employment outcomes.**

Although GED receipt and increased basic skills appear to have positive effects on the earnings of welfare recipients, those effects appear to be much stronger when spells of adult education and receipt of a GED are followed by enrollment in postsecondary education or training programs. The orientation of many of these programs toward specific jobs and career opportunities may be a factor in explaining these programs' apparent benefits. Participation in postsecondary education or training carries a price in terms of lower short-term earnings, but the longer-term effects of these services are substantial. Especially after a long spell of participation in adult education, it makes sense to cap off this investment with some college or vocational skills training.

- **The analyses presented in this report address important questions about adult education provided to welfare recipients, but many important questions remain, especially regarding the quality of participation in adult education, the appropriateness of adult education services provided to welfare recipients, and the possible benefits of education and training for those who do have an education credential when they enter the programs.**

In this report, we present program effects on participation in adult education and key educational outcomes. We relate these effects to one another and to the employment and welfare outcomes of those being targeted by the programs. However, although we assessed the effects of participation in adult education, we did not capture all the reasons for *non*participation. Although we measured the effects of earning a GED, we do not know why so many participants never received this credential. Answers to both of these questions, and others like it, may provide a greater understanding of the "quality" of participation (that is, the actual commitment to learning manifested by students who were coerced to participate in adult education) and by the quality of the instruction (that is, the appropriateness of teaching materials and techniques for the welfare recipients in these programs). The study data did not capture either one of these "quality" measures accurately, and it is therefore difficult to say whether simply increasing enrollment in adult education programs beyond current levels would significantly improve the outcomes of these programs. More detailed data about the quality of students' program experience must be collected to address these questions, and more systematic comparisons of the different types of adult education programs that serve welfare recipients are needed.

The analyses presented in this report are limited to welfare recipients who did not have a high school diploma or GED and were considered to be in need of basic education. No parallel study was conducted to examine the effects of education and training on welfare recipients who did have an education credential. Our findings do not generalize to this group, and additional research may be needed to assess whether and how additional education and training benefits welfare recipients who are less disadvantaged academically.

Chapter 2

Characteristics of Adult Education in Welfare-to-Work Programs and the Effects of Education-Focused Welfare-to-Work Programs on Educational Attainment and Achievement

I. Introduction

The purposes of this chapter are to describe the adult education that was provided as part of three education-focused welfare-to-work programs that were run in the early to mid 1990s and to present estimates of the effects of the programs on educational outcomes.[1] The chapter uses data from three education-focused mandatory welfare-to-work programs studied as part of the National Evaluation of Welfare-to-Work Strategies (NEWWS Evaluation).[2] As mentioned in Chapter 1, three of the sites—Atlanta, Georgia; Grand Rapids, Michigan; and Riverside, California—operated two programs side by side: an employment-focused, or Labor Force Attachment (LFA) program; and an education-focused, or Human Capital Development (HCD) program. The education-focused programs emphasized skills-building through education and training as a precursor to employment, based on the belief that an upfront investment in the skills levels of welfare recipients allows them to obtain higher-paying and more secure jobs. In the education-focused programs, most people were first assigned to education or training; adult education was the most common activity because of the generally low educational attainment of the enrollees at program entry.[3]

This chapter answers the following questions:

- What was the adult education like that was provided as part of the mandatory education-focused welfare-to-work programs in Atlanta, Grand Rapids, and Riverside?
- What were the welfare-to-work programs' effects on the use of adult education services?
- What were the programs' effects on educational attainment and achievement?
- How did these effects vary for different types of welfare recipients (that is, subgroups of sample members)?

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[1] Recall from Chapter 1 that adult education encompasses adult basic education (ABE) classes, General Educational Development (GED) classes, high school completion classes, and English as a Second Language (ESL) classes.

[2] The NEWWS Evaluation is a study of 11 welfare-to-work programs run in seven sites. The evaluation is being conducted by the Manpower Demonstration Research Corporation, under contract to the U.S. Department of Health and Human Services, with support from the U.S. Department of Education.

[3] See Hamilton et al., 1997, for a detailed comparison of the two program approaches in these three sites.

The answer to the first question is essentially a description of the adult education provided in the three programs, covering such issues as the links between the education institutions and the welfare-to-work programs, the classes' educational content and methods, and the teachers' qualifications. The next three questions shift the focus to the broader context of the welfare-to-work programs; the answers to the questions are based on analyses of various program effects. As discussed in Chapter 1, the NEWWS Evaluation used a random assignment research design. Outcomes for the control group represent what would occur in the absence of a mandatory welfare-to-work intervention. The differences between outcomes for the program group and the control group represent estimated effects, or *impacts*, of the programs.

Conclusions from a random assignment experiment are reliable and can provide important information on welfare recipients' participation in adult education and the effects of mandatory welfare-to-work programs on recipients' educational outcomes. It should be noted, however, that the analysis in this chapter does not attempt to isolate the effects of the adult education itself; rather, it focuses on the effects of the entire package of the welfare-to-work programs' services and mandates described below. Impacts are averaged over the full sample—those who participated in adult education as well as those who did not. This can "dilute" the effects of adult education. (Chapter 3 explores more directly the connections between adult education and various outcomes.)

The fact that the adult education examined here was provided in the context of mandatory welfare-to-work programs raises two important considerations. First, adult education was only part of the program treatment; the programs also included other activities, such as job search and vocational training, and they offered support services, such as child care assistance and counseling. In addition, the programs were "mandatory"—they could impose a financial penalty on those who did not comply with program requirements. The mandates and financial sanctions, however, remained in effect only as long as a person was on welfare; someone who left the rolls could terminate her participation in all program activities, including adult education, without any penalty from the welfare-to-work program. Second, the programs involved adults in education who would not otherwise have participated. Whereas "traditional" students in adult education programs enroll voluntarily and can therefore be presumed to be motivated to learn, such motivation cannot be assumed in the case of the students studied here.

Typically, adult education programs have targeted adults without a high school diploma or General Educational Development (GED) certificate.[4] Therefore, this chapter focuses on this portion of the sample in each site (adults with a high school diploma or GED were also randomly assigned to these programs, but they are not considered here). In other words, the *full sample* for this chapter is those who did not have a high school diploma or GED at random assignment.

II. Summary of the Findings

- **Generally, the inclusion of welfare-to-work program students did not greatly affect the operations, curricula, or teaching methods of the adult education programs.**

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[4] Pauly, 1995.

Although the education programs made some changes to accommodate welfare-to-work program regulations and students, the adult education provided as part of the three welfare-to-work programs was similar to that provided to students who were not part of a welfare-to-work program. Most education institutions included some work preparation activities in classes, but they emphasized traditional adult education content (academic topics and preparation for the GED tests). The welfare-to-work programs, however, provided their students with additional support, such as child care.

- **Adult education in the three welfare-to-work programs was provided by a wide variety of education institutions. Although many differences exist when comparing the institutions, the differences do not appear to be systematic.**

In other words, no distinct, separate categories of providers emerge based on the characteristics examined in this chapter. Likewise, differences exist across the three sites, but they are not systematic.

- **Like most mandatory education-focused welfare-to-work programs that have been studied, the programs in Atlanta, Grand Rapids, and Riverside substantially increased participation in adult education.**

Without the intervention of a mandatory welfare-to-work program, about one-fifth (18 percent) of the control group took part in adult education classes over a two-year follow-up period. The programs studied here more than doubled this participation rate: one-half (50 percent) of the program group participated in adult education. The programs more than tripled the time spent in adult education: control group members spent an average of 68 hours in classes, compared with 244 hours for program group members—an increase of 176 hours. This increase in hours participated should be considered in the context of education outside the adult education system. As mentioned earlier, this increase is substantially less than the instruction provided in one year of high school and therefore should not be expected to produce dramatic changes in educational outcomes. Presented in terms of months, this means that control group members participated in adult education for an average of 1.2 months, while program group members participated for an average of 3.7 months—an increase of 2.5 months.

- **Mirroring the pattern found in other studies, the three welfare-to-work programs discussed in this chapter increased educational attainment but had no effect on educational achievement.**

Across the three sites, 4 percent of the control group received a high school diploma or GED during the two years following random assignment, compared with 11 percent of the program group—a modest increase of 7 percentage points. The programs did not, however, increase scores on standardized reading and math tests. Various factors may make it difficult for programs to produce test score gains. First, many who were in the programs did not receive any adult education; it is possible that education participants did achieve test score gains but that their gains are masked by outcomes for nonparticipants. Second, not all adult education is designed to increase reading and math achievement. Third, standardized tests may not measure the skills that were learned. Fourth, administration of achievement tests as part of a survey, rather than in a classroom setting, may somewhat limit the reliability of test results, making it more difficult to find statistically significant program effects.

- **The findings presented in this chapter show that mandatory welfare-to-work programs can substantially increase participation in adult education and educational attainment for many different types of people.**

The programs increased participation for all 20 subgroups examined, and they increased high school diploma or GED attainment for 17 of the 20 subgroups.

- **There is no systematic variation in participation impacts across the subgroups.**

A previous study (an evaluation of California's GAIN program) found that the welfare-to-work program generally had a greater effect on the amount of education received by the more disadvantaged segment of the caseload. Researchers proposed that this occurred in part because more disadvantaged people tended to spend more time on AFDC and were available to participate in activities for a longer period.[5] In the present study, this pattern was found for only some of the subgroups.

- **No clear relationship exists between the impact on hours of participation in adult education and impacts on educational attainment or educational achievement.**

As was found in the GAIN evaluation, subgroups with larger increases in hours of participation did not necessarily have larger gains in attainment or achievement. Regarding educational attainment, this probably reflects that many persons who received a high school diploma or GED were close to receiving one when they entered the programs, which then provided the necessary support services, the moral support, or the "push" needed to obtain the credential. Thus, their stays in education likely were relatively brief. Those who remained in education for extended periods were probably far from receiving the credential. (Using more sophisticated statistical methods, Chapter 3 explores further the relationship between length of participation and educational attainment and achievement.)

- **As has been found in previous research, the programs studied here produced much larger impacts on educational attainment for individuals with higher preprogram achievement levels than for those with lower preprogram achievement levels.**

The programs increased the receipt of a high school diploma or GED by 16 percentage points among those entering with high reading scores, and by 18 percentage points among those entering with high math scores (compared with impacts of 3 percentage points among those with low reading and low math scores). Said another way, the welfare-to-work programs helped many individuals with higher skills levels get education credentials.

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[5] Martinson and Friedlander, 1994.

- **Notably, the programs produced major impacts for individuals who were not motivated to attend school.**

The programs substantially increased participation and receipt of education credentials for those who at program entry said they did not like school and/or did not plan to go to school.

- **The programs produced smaller impacts for individuals with many family or personal problems than for those in most other subgroups.**

The programs produced somewhat smaller participation increases for those who had family or personal problems (such as health or emotional problems) and did not increase educational attainment for this subgroup.

III. A Brief Review of Prior Research

Before discussing results from the three programs studied as part of the NEWWS Evaluation, it is useful to briefly review relevant prior research. This section summarizes findings on the adult education that has been provided to welfare recipients as part of education-focused welfare-to-work programs, participation in adult education in these programs, and various effects of these welfare-to-work programs.[6]

A. A Description of Adult Education for Welfare Recipients

For decades adult educators have placed a high priority on serving people on welfare. Until the late 1980s, however, few direct linkages existed between adult education and welfare. In 1988, the Family Support Act (FSA) for the first time closely linked adult education with welfare and provided substantial benefits (including child care and transportation support)as well as obligations for adult education students.Welfare-to-work programs that operated under the FSA referred many people who were receiving cash assistance under the Aid to Families with Dependent Children (AFDC) program[7] to adult education, but many of these programs did not contribute to the funding of the education programs; this may have limited the nature and extent of coordination between the adult education and welfare-to-work systems. Although coordination on referrals and attendance monitoring was widespread, coordinated efforts to improve program quality and effectiveness appear to have been less common.

Research has identified considerable diversity in the goals of adult educators who served people on AFDC: some educators preferred approaches that tie adult education to work, while others focused on the traditional goals of higher reading and math achievement, English language fluency, and the receipt of the GED credential. Sometimes the goals of adult educators differed from the goals of welfare-to-work program operators or from the goals of adult students. One analyst[8] pointed out that failing to address such disagreements can lead to a failure to develop educational approaches that meet the various goals and can result in services that do not meet students' needs.

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[6] This section summarizes key points from a previous NEWWS Evaluation publication, *The JOBS Evaluation: Adult Education for People on AFDC: A Synthesis of Research* (Pauly, 1995).

[7] Until the 1996 welfare reform law, AFDC was the nation's principal safety net for poor families.

[8] Pauly, 1995.

Reflecting diverse goals, the content of adult education programs serving people on AFDC varied substantially. Many programs based their educational content primarily on publisher's textbooks, workbooks, and other materials.While many programs went beyond a "publisher-driven, 'plain vanilla' approach,"[9] the prevalence of innovation in educational content is unknown. Studies have identified various kinds of work-related content in adult education programs serving AFDC recipients—such as punctuality, styles of dress for the workplace, career planning activities, and job search instruction—although it is unclear how many programs included this type of content. Many adult education programs incorporated motivational and supportive content, such as self-esteem-building activities and study skills instruction.

Research has not identified how widespread particular methods of teaching and learning are in adult education programs that serve welfare clients, and the available knowledge base is too limited to evaluate the effectiveness of specific instructional approaches and innovations. Based on the view that students learn in different ways, some programs have used multiple methods, often combining both group and individual activities, computer-assisted instruction, tutoring and other one-on-one instruction, and cooperative learning.

The schedules of adult education programs serving people on AFDC also varied substantially: some programs offered 25 or more hours of instruction each week, while others offered fewer than 10 hours per week. These differences reflect differences in programs' resource levels and intensity of services.

Given the diversity of goals for welfare clients who participate in adult education, it is not surprising that there is little published information on the quality of adult education services for AFDC recipients.

An earlier NEWWS Evaluation report profiled adult education programs in four communities that were considered to be serving welfare clients "in innovative and promising ways."[10] The four programs, which were not in the sites formally involved in the NEWWS Evaluation, were chosen by MDRC researchers and U.S. Department of Education staff based on perceptions of innovation and quality in the education community, interviews, and in-person visits. Case studies of these programs uncovered a number of "promising practices" across the programs:[11]

- a well-defined mission
- separate classes specifically for students from welfare-to-work programs
- skilled, experienced teachers
- an emphasis on staff development

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[9] Pauly, 1995, p. 20.

[10] Quint, 1997, p. vi.

[11] See Quint, 1997, for a discussion of how the programs were chosen, descriptions of the programs, and more detail on these "promising practices."

- varied instructional approaches that involve active learning
- frequent communication about students' progress between educators and welfare-to-work program staff
- an emphasis on regular attendance, with aggressive follow-up of absences
- relatively intensive class schedules (at least 20 hours per week)
- a high degree of teacher-student and student-student interaction

It is important to note that this list was generated from operational wisdom rather than from statistical evidence that these practices consistently affect positive outcomes. (This chapter will examine some of these dimensions among adult education providers in Atlanta, Grand Rapids, and Riverside. Chapter 3 directly explores the link between some of these facets and increased educational attainment and achievement.)[12]

B. Participation in Adult Education in Education-Focused Welfare-to-Work Programs

Welfare-to-work programs have been an effective pathway to adult education; previous studies found that programs substantially increased levels of participation in adult education classes. Furthermore, students who participated in adult education classes as part of a welfare-to-work program received considerably more hours of service than other adult education students. Among adult education programs funded by the Adult Education Act of 1966, students received a median of 58 hours of service;[13] in adult education programs tied to welfare-to-work programs, participants typically received at least 100 to 200 hours of service. Two factors may help explain this finding: federal regulations in place before the 1996 welfare law created incentives for states to assign a substantial fraction of welfare-to-work program participants to education services for at least 20 hours per week, and the prospect of financial sanctions in mandatory programs presumably induced some people to remain in education longer than they would have on their own.

These positive effects on the length of time spent in adult education classes must be considered in the context of education outside the adult education system: one year of high school entails more than 800 hours of instruction.[14] In general, 100 or 200 hours of adult education should not be expected to produce dramatic changes in educational attainment or achievement.

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[12] Note, however, that the evaluation of adult education providers in Atlanta, Grand Rapids, and Riverside was designed before the completion of the case studies. While some of the provider characteristics examined in the three sites are similar to some of the identified "promising practices," they are not identical, and the evaluation was not designed to test these practices.

[13] Development Associates, 1994.

[14] Researchers at the National Center for Education Statistics found that the average high school student received 25.2 credits of instruction in 1998, of which 18 were academic credits (U.S. Department of Education, 2000). Based on this information, MDRC researchers calculated the total number of hours of academic instruction received by an average high school student to be approximately 3,240 hours. (The calculation assumes that 18 credits are equivalent to 18 one-year courses of approximately one hour per day and that an average school year has approximately 180 days.) Dividing 3,240 hours by 4 years results in an estimate of 810 hours of academic instruction annually.

Despite the fact that welfare-to-work programs increased hours of adult education participation beyond what normally would have occurred, researchers have identified a few factors that limit the amount of education that welfare clients received. First, poor attendance was a major problem in adult education programs that served AFDC recipients (although not necessarily a bigger problem than for adult education programs serving those who were not receiving AFDC).[15] Second, evidence suggests that many persons exited adult education before completing the program,[16] either because they became employed or left the welfare rolls for other reasons or because they became ill or faced other problems that caused them to drop out.[17]

C. Effects of Education-Focused Welfare-to-Work Programs

1. Receipt of a GED Certificate. Welfare-to-work programs emphasizing adult education have consistently and significantly increased the receipt of a GED certificate. Studies including analyses of preprogram achievement levels found that people with higher preprogram achievement levels had substantially larger impacts on GED attainment than those with lower achievement levels. Adults with lower achievement levels often took ABE or ESL classes that were focused on improving reading, language, and math skills, rather than GED classes that prepared them to take the GED test. Also, students in GED classes who had lower achievement levels were generally less successful than other participants in attaining the GED credential.

Research has also found that these programs left a substantial proportion without a GED or high school diploma. A number of factors help explain this: (1) importantly, many persons in these programs did not participate in adult education and were not expected to obtain a GED certificate; (2) many of the welfare-to-work programs also stressed noneducation activities and allowed participants to seek employment; (3) as mentioned, ABE and ESL programs generally are not intended to help students obtain these credentials; and (4) some program exits occurred when individuals left AFDC. Thus it may not be a reasonable goal that *all* welfare recipients who lack a high school diploma or GED certificate get a credential.

2. Educational Achievement. Raising reading and math test scores appears to be a challenging goal for welfare-to-work programs providing adult education. A 1995 synthesis of research found that among studies that examined achievement, only two of nine programs raised test scores.[18] Analogous to the pattern in the GED attainment results, the impacts on achievement in one of these studies were concentrated among individuals with higher preprogram achievement levels.[19]

It is important to note, however, that the tests typically used to assess the functional reading and math skills of adults are not aligned with the curricula used in adult education classes. Tests may or may not measure the skills that were learned by the adult education students who were studied. Additional caveats, similar to those about GED attainment, apply to the test score results. First, many persons in the studies' samples did not receive any adult education. Even if adult education participants achieved substantial test score gains, their gains could be diluted by the outcomes of others who did not receive adult education. Second, not all adult education programs are designed to increase achievement in reading and math. GED preparation classes typically concentrate on helping students learn the discipline-based information and skills that are the subject of the GED tests. These classes often assume that students already possess adequate reading skills, and they generally are not intended or expected to affect reading achievement levels. Although one of the GED tests covers math, the test concentrates on algebra and geometry, rather than on the lower-level math skills that are measured in many achievement tests for adults.

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[15] Teachers providing adult education in Atlanta, Grand Rapids, and Riverside rated the attendance of welfare-to-work program students as similar to, or better than, the attendance of students not in the program (Quint and Walter, 1995).

[16] About one-quarter of teachers in Atlanta, Grand Rapids, and Riverside estimated that half or more of their welfare-to-work program students stopped attending class before completing it (Quint and Walter, 1995).

[17] Quint, 1995.

[18] Pauly, 1995.

[19] This was the GAIN program in San Diego; see Martinson and Friedlander, 1994.

3. Employment and Welfare Receipt. The relationship between adult education in welfare-to-work programs and economic impacts is complex. Many welfare-to-work programs have increased employment and earnings and have reduced welfare receipt for clients generally regarded as needing adult education, but the role of adult education in producing these impacts is unclear. (Chapter 4 of this report explores the effects of receiving a GED and of increased educational achievement levels on subsequent earnings patterns. Then Chapter 5 examines the overall impacts of the three education-focused programs—along with the other eight programs in the NEWWS Evaluation—on earnings and welfare payments.)

IV. A Description of the Adult Education Provided as Part of the Education-Focused Programs in Atlanta, Grand Rapids, and Riverside

This section primarily uses data collected from the major institutions providing adult education to welfare-to-work program participants in Atlanta, Grand Rapids, and Riverside. The data were gathered through surveys administered to teachers and through interviews with administrators at the same institutions.[20] (See Appendix A for a description of these and other data sources used in this chapter and for the associated sample sizes.)

Adult education in the three welfare-to-work programs was provided by a wide variety of education institutions. (This section uses interchangeably the terms *education institution* and *education provider*.) Although these institutions differed in many ways, the differences do not appear to be systematic. In other words, no clear, distinct categories of providers emerge based on the characteristics examined in this chapter. Likewise, differences in provider characteristics emerged across the three sites but were not systematic. (Chapter 3 examines the role of some of the provider characteristics discussed here in affecting students' educational attainment and achievement.)

The adult education programs in Atlanta, Grand Rapids, and Riverside were solid, more or less typical adult education programs. Generally, the inclusion of students who were in a welfare-to-work program did not greatly affect operations, the curricula, or the teaching methods of the adult education programs. In Riverside, the link between the welfare-to-work program and the education programs was a bit stronger, largely because the education programs were directly funded by the welfare-to-work program. Across the sites, almost no classes served only welfare-to-work students, and typically these students were in the minority. The programs provided some instruction on work preparation, but most focused primarily on traditional educational content. Classes spent more time on reading and writing activities than on math, and most programs provided at least a moderate degree of individual attention and emphasized personal relationships. On average, classes were smaller in Atlanta and Grand Rapids than in Riverside.

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[20] Some of this information was presented in Hamilton et al., 1997. It is included here to provide a complete description of the education institutions.

A. Major Institutional Education Providers

All sites relied principally on adult education programs operated through local school systems, although Atlanta also used community-based nonprofit organizations. The size of these institutions varied considerably in every site: some schools were small, with annual student enrollments of 120 or fewer, and some were large, with annual student enrollments of several thousand.

B. Types of Adult Education Offered

The three sites used each type of adult education to varying degrees. Because the sites relied mostly on existing educational resources within their communities, state and local education policies primarily determined what kinds of classes were available. The state of Michigan funded high school completion but not GED programs; consequently, in Grand Rapids more students were enrolled in high school completion than in Atlanta or Riverside. Most classrooms contained more than one type of student. For example, high school completion and GED students might attend the same class in Grand Rapids, and ABE and GED students might be in a single class in Riverside.

C. Links Between Adult Education and the Welfare-to-Work Programs

The Riverside welfare-to-work program was unique among the three sites in that it negotiated contracts with *all* the schools serving welfare-to-work clients, using welfare-to-work program funds to help pay for these adult education classes. Atlanta and Grand Rapids, in contrast, referred people to education providers funded by sources outside the welfare-to-work program (usually state and local education departments). The adult education programs in these two sites relied heavily on revenue from state education agencies (sometimes augmented by funds from the state welfare departments) to fund instruction to welfare-to-work participants.

Riverside took advantage of its resources and its contracting authority to influence aspects of the education its program members received. The Riverside program established precise criteria for determining how clients would be placed in different education programs (ABE, GED, or ESL) and the duration of these assignments. The contracts between the welfare department and the education providers included incentive payments for providers that succeeded in getting clients to make progress in and complete their education assignments. Compared with Riverside, the Atlanta and Grand Rapids welfare-to-work programs gave much more discretion to education providers. Once the welfare-to-work program staff determined that individuals needed adult education and referred them to a school, the education providers' staff were responsible for placing clients in an appropriate ABE, GED, ESL, or high school completion class and for determining when students should exit. (See section E below, on exit standards, for details on how the welfare-to-work programs influenced exits from classes.)

A majority of adult education staff serving welfare-to-work program participants in Atlanta, Grand Rapids, and Riverside indicated that they increased the number of days or hours that they offered classes in order to accommodate welfare-to-work students. These increases were often driven by the programs' need to assign clients to an average of 20 hours of instruction and study time per week, as required by federal regulations. A few programs also enhanced the kinds of services they offered, adding, for instance, additional counseling for welfare-to-work students or more vocational or job-readiness instruction. Across the three sites, program administrators in Atlanta reported the greatest expansion in the services they offered, but teachers in Atlanta indicated that the changes were modest.

Table 2.1 summarizes some characteristics of the education providers using data from the teacher survey. Provider-level measures are the average of the responses among the teachers associated with a specific provider. Teachers' survey responses corroborated that the link between the welfare-to-work program and the education institutions was stronger in Riverside than in the other sites. Teachers were asked a series of questions on communication with the welfare-to-work program, including how often they talked with program staff and the likelihood they would report student attendance problems to program staff. (See Appendix B for a list of the survey questions used to create this scale and the other scales presented in the table.) As Table 2.1 shows, in Atlanta and Grand Rapids one-third or fewer of the education providers had a moderate or high level of communication with the welfare-to-work program. In Riverside, however, almost three-fourths of providers had a moderate or high level of communication with the program. Recall that frequent communication about students' progress between educators and program staff was identified as a "promising practice." Based on teachers' responses, this occurred more frequently in Riverside than in the other sites.

Another "promising practice" mentioned earlier is providing separate classes for welfare-to-work students. No education institution in the three sites exclusively served welfare-to-work clients, and almost all classes included students from welfare-to-work programs as well as other adult education students. As Table 2.1 shows, fewer than 10 percent of providers in Grand Rapids and Riverside had a high proportion (70 percent or more) of welfare-to-work program students in their classes. Atlanta providers had classes with higher proportions of welfare-to-work students, but in only 29 percent of institutions did these students make up 70 percent or more of the typical class.

D. Educational Content and Methods

The education institutions, on average, spent more time on reading and writing activities than on math activities. As Table 2.1 shows, between 0 and 27 percent of institutions spent only a little time on reading and writing, compared with between 14 and 73 percent of institutions that spent a little time on math. According to teachers' responses, instruction in Atlanta was most evenly balanced between reading and writing and math activities: 100 percent of providers spent a moderate amount or a lot of time on reading and writing, and 86 percent spent a moderate amount or a lot of time on math.

Skills tested in the Test of Applied Literacy Skills (TALS) document literacy test were taught in classes in Atlanta and Riverside to a greater degree than in classes in Grand Rapids. Skills tested in the Comprehensive Adult Student Assessment System (CASAS) math test were taught in classes in all the sites. Despite the generally greater emphasis on reading instruction, providers did not emphasize reading test items more than they emphasized math test items. The

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Table 2.1

Characteristics of Major Education Institutions Providing Adult Education to Sample Members in Three Education-Focused Programs

Scale or Measure	Atlanta (%)	Grand Rapids (%)	Riverside (%)
Link to Welfare-to-Work Programs			
Communication between program and education institution staff			
High	28.6	0.0	9.1
Moderate	0.0	33.3	63.6
Low	71.4	66.7	27.3
Average proportion of welfare-to-work program students in the classroom			
70% or more	28.6	6.7	9.1
30% - 69%	42.9	33.3	18.2
Less than 30%	28.6	60.0	72.7
Educational Content and Methods			
Time spent on reading and writing activities			
A lot	42.9	13.3	18.2
A moderate amount	57.1	66.7	54.6
A little	0.0	20.0	27.3
Time spent on mathematics activities			
A lot	28.6	6.7	0.0
A moderate amount	57.1	33.3	27.3
A little	14.3	60.0	72.7
Degree to which skills tested in the TALS document literacy test were taught in class			
High	71.4	21.4	70.0
Moderate	28.6	21.4	20.0
Low	0.0	57.1	10.0
Degree to which skills tested in the CASAS math test were taught in class			
High	71.4	64.3	100.0
Moderate	14.3	21.4	0.0
Low	14.3	14.3	0.0
Education/work link			
Strong	0.0	0.0	9.1
Moderate	100.0	60.0	63.6
Weak	0.0	40.0	27.3
Proportion of teachers who primarily used a mix of traditional and nontraditional teaching methods[a]			
50% or more	28.6	53.3	63.6
Less than 50%	71.4	46.7	36.4

(continued)

Table 2.1 (continued)

Scale or measure	Atlanta (%)	Grand Rapids (%)	Riverside (%)
Classroom Environment			
Degree of individual attention			
High	14.3	0.0	0.0
Moderate	71.4	86.7	90.9
Low	14.3	13.3	9.1
Emphasis on personal relationships			
High	85.7	86.7	45.5
Moderate	14.3	13.3	54.6
Low	0.0	0.0	0.0
Average number of students attending class			
18 or fewer	100.0	93.3	18.2
More than 18	0.0	6.7	81.8
Teachers' Characteristics and Benefits			
Average proportion of teachers working full time[b]			
50% or more	71.4	86.7	36.4
Less than 50%	28.6	13.3	63.6
Average years of teaching experience			
10 or more years	57.1	93.3	100.0
Less than 10 years	42.9	6.7	0.0
Percentage of teachers with master's degrees or higher education credentials			
50% or more	57.1	60.0	63.6
Less than 50%	42.9	40.0	36.4
Receipt of fringe benefits[c]			
High	14.3	53.3	9.1
Moderate	71.4	46.7	81.8
Low	14.3	0.0	9.1
Teachers' Views			
Average rating of teaching materials and equipment			
High	71.4	53.3	81.8
Moderate	28.6	46.7	18.2
Low	0.0	0.0	0.0
Average rating of classroom morale			
High	100.0	86.7	90.9
Moderate	0.0	13.3	9.1
Low	0.0	0.0	0.0
Self-perceived teacher effectiveness			
High	100.0	86.7	90.9
Moderate	0.0	13.3	9.1
Low	0.0	0.0	0.0
Number of education institutions	7	15	11

(continued)

Table 2.1 (continued)

SOURCE: MDRC calculations from data obtained through a survey of adult education teachers in each major education institution serving welfare-to-work clients in the fall of 1993.

NOTES: An education institution's score on the above scales and measures represents the average of its teachers' scores. In Atlanta, 24 teachers were surveyed; in Grand Rapids, 79; and in Riverside, 45. For an enumeration of composite measures presented above, see Appendix B.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

[a] In this scale, traditional teaching methods are defined as published textbooks, workbooks, or worksheets, and nontraditional teaching methods are novels or short stories, newspapers or magazines, materials written by teachers or by other staff members at the institution, materials developed by students, materials that are about the students' home and community environment, educational games, audio visual equipment, computers, and job application forms and other real-life documents. Teachers were classified as primarily using a mix of traditional and nontraditional methods if they indicated that the two materials they used most commonly in class included one traditional and one nontraditional method.

[b] Full-time employment is defined as working 30 hours per week or more.

[c] Fringe benefits are defined here as health benefits, vacation benefits, sick leave, and retirement benefits. In the scale above, "low" indicates that teachers at education providers, on average, received none of these benefits; "moderate" indicates that teachers received between one and three of these benefits; and "high" indicates that teachers received more than three of these benefits.

questions used to create the "test items" scales simply capture whether the skills covered by the test questions were taught in the classroom, not whether teaching these skills consumed most of the class time.[21] A class that spent a majority of time on reading could still have spent some time on skills covered in the math test.

Most institutions attempted to integrate work preparation into their education curriculum, but almost no institutions did this to a high degree (that is, reported providing a strong link between education and work). Activities used in classes to prepare students for work included composing letters to hypothetical employers, practicing writing résumés, and using reading material about work situations.

As mentioned, an earlier study concluded that using a variety of teaching methods may be more effective than relying solely on traditional adult education methods (based on published textbooks, workbooks, or worksheets).[22] As Table 2.1 shows, more than half the providers in Grand Rapids and Riverside had teachers who used a mix of traditional and nontraditional teaching methods, compared with less than a third in Atlanta. For this measure, nontraditional teaching methods include novels or short stories, newspapers or magazines, materials developed by teachers or students, and computers (see the footnote for Table 2.1 for a complete list of nontraditional methods used). Teachers were classified as "primarily" using a mix of traditional and nontraditional methods if they indicated that their two most commonly used materials included one traditional and one nontraditional method.

E. Exit Standards

As previously noted, Riverside established criteria for the duration of education assignments, and it offered the education providers incentive payments for getting clients to complete their assignments. Underlying these standards was a desire that program participants learn quickly, acquire just enough skills to move up to the next class level, and then leave the program's education component. Riverside welfare-to-work staff, for example, were unlikely to allow students starting out in an ABE or ESL class to remain in education until they earned a GED certificate. Rather, ABE or ESL students would have been permitted to stay in school only until they achieved a target score on an educational achievement test specified by the state welfare agency. After that they were expected to participate in program activities more directly related to finding entry-level employment. Riverside welfare-to-work staff generally expected participants in ABE, GED, and ESL to complete their education assignment within 6 to 12 months.

In contrast, in Atlanta and Grand Rapids, both school staff and welfare-to-work staff usually encouraged people beginning in ABE or ESL to stay in education until earning their high school diploma or GED certificate. Education providers in Atlanta and Grand Rapids sometimes recommended that individuals who had attained a high school diploma or GED certificate remain in an adult education classroom for a bit longer if they believed individuals were weak in an academic subject area or needed to build more confidence before moving into college or vocational training. Exit criteria, therefore, were based much more on teachers' assessments than on predetermined standards or test scores. Adult education administrators estimated that it could take as long as three years for students to complete an education program in their schools, although welfare-to-work case managers in the two sites were asked to limit education classes to two years.

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[21] Teachers were asked to examine a series of questions that appear on the TALS document literacy test and the CASAS math test. They were then asked whether the skills and content area covered by the test questions were taught in their class.

[22] Quint, 1997.

F. Classroom Environment

The four programs identified as "innovative and promising" in the earlier NEWWS Evaluation case studies were characterized by a high degree of teacher-student interaction. Most programs in Atlanta, Grand Rapids, and Riverside provided only a moderate degree of individual attention when instructing students (indicated by such practices as one-on-one instruction and developing lesson plans for individual students); few provided either a high or low degree. (See Table 2.1.) Most providers did emphasize the importance of personal interaction between students, their teachers, and their fellow classmates; this was particularly true in Atlanta and Grand Rapids.

Classes in Riverside, on average, were much larger than classes in the other two sites. Most classes in Riverside had more than 18 students, whereas all classes in Atlanta and most in Grand Rapids had 18 or fewer.[23] The differences in class size may partly reflect differences in how classes were organized at the three sites. Although a large majority of classes at all the sites operated on a fixed class schedule, Riverside had more classes than Atlanta or Grand Rapids that operated on a "drop-in" basis. The drop-in centers provided individual workstations that had computers and other learning material and could accommodate more students than could a formal classroom setting that operated on a fixed schedule.

On average, classes met four days per week in Atlanta and Riverside and three days per week in Grand Rapids. The average number of class hours per week varied across the sites: classes in Atlanta met for an average of 16 hours per week; in Grand Rapids, 10 hours per week; and in Riverside, 17 hours per week (not shown in the table). The differences do not necessarily reflect different time spent overall in classroom activities. For example, Grand Rapids students often were assigned to more than one class—a product, possibly, of a curriculum that contained numerous high school courses. Adult education classroom time often was supplemented by several hours of independent study in all three sites. Few classes operated on a strict calendar; instead, students could enter and exit at any point during the school year.[24]

G. Teachers' Characteristics, Benefits, and Views

In Atlanta and Grand Rapids, at least half the teachers were employed full time at the vast majority of providers, whereas in Riverside only about a third (36 percent) of providers had more than half their teachers working full time (at least 30 hours per week). Having skilled, experienced teachers is one of the "promising practices" noted earlier. Most teachers had at least 10 years of teaching experience, and in the majority of providers in each site at least half had a master's degree or higher. Most providers offered teachers at least some fringe benefits—including health, vacation, and retirement benefits as well as sick leave. More providers in Grand Rapids than in the other sites offered a high level of benefits.

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[23] Average class attendance in Atlanta was 13 students; in Grand Rapids, 14 students; and in Riverside, 28 students (Hamilton et al., 1997).

[24] As noted earlier, a majority of the education institutions serving welfare-to-work program clients in Atlanta, Grand Rapids, and Riverside indicated that they increased the number of days or hours that they offered classes in order to accommodate these students. A study of adult education programs found that the mean weekly hours per week that clients attended class varied from 5 to 13 hours per week (Development Associates, 1992).

Generally, teachers were satisfied with the materials and equipment available to them, including the physical plant, books, workbooks, computers, and software. MDRC field researchers usually shared this impression. Classroom morale was high in each of the sites, and the teachers perceived themselves to be effective.

V. Characteristics of Welfare Recipients Without a High School Diploma or GED Certificate in the Three Sites

Expanding the focus from the adult education providers to the welfare-to-work programs, the remainder of this chapter describes the characteristics of sample members in the study and discusses program effects on their participation in adult education, their receipt of educational credentials, and their educational achievement. Most analyses are presented for a single, pooled research sample across the three sites.[25]

Initially, all those randomly assigned for the NEWWS Evaluation in Atlanta, Grand Rapids, and Riverside were mandated to participate in the welfare-to-work program. After random assignment, those assigned to the control group were excused from participating, while those in the program group faced possible reductions in their welfare grant if they failed to participate. Our sample is limited to single parents.[26] Among single parents, AFDC applicants and recipients were required to participate if their youngest child was 3 or older in Atlanta and Riverside, and 1 or older in Grand Rapids,[27] and if they did not meet certain exemption criteria.[28] All sample members in Atlanta and Riverside were age 20 or older, while the Grand Rapids sample also includes individuals of 18 and 19.

As mentioned previously, this chapter examines the experiences of participants who are the typical target group for adult education: those without a high school diploma or GED certificate (at program entry). In Riverside, some sample members who had a high school diploma or GED certificate were included in the sample, namely, those who had low reading or math skills or were not proficient in English.[29] For ease of reference, all these individuals are referred to as those *without a high school diploma or GED*, or *nongraduates*. Among education-focused welfare-to-work programs included in the NEWWS Evaluation, this nongraduate sample comprised 39 percent of those in Atlanta, 40 percent of those in Grand Rapids, and 100 percent of those in Riverside.[30]

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[25] Programs were weighted equally in the pooled analyses.

[26] In Riverside, the primary wage earner in two-parent families was randomly assigned, but this portion of the sample is not analyzed in this chapter.

[27] Federal regulations mandated participation from parents with children 3 and older and gave states the option to mandate participation from parents with children as young as 1.

[28] Federal exemption reasons included working 30 hours or more per week, having a disabling illness, being in at least the second trimester of pregnancy, or living in a remote area that made program activities inaccessible. Grand Rapids (Michigan) had some additional state-specific exemption reasons: if a recipient had three or more children under age 10, had been within the last five years a resident of a mental institution, had been using prescribed medication for mental illness, or had been enrolled in a rehabilitation program for at least 15 hours per week.

Table 2.2 presents key demographic, educational, and employment information for the nongraduate samples pooled across the three education-focused programs. (The pooled numbers, weighting each site equally, represent the average for each measure across the three sites. Appendix Table C.1 shows these characteristics broken down by site.) As the table shows, most sample members are women, and their average age at the beginning of the study was 31 years. About half (49 percent) of the sample is black, about one-third is white (31 percent), and roughly one-fifth (18 percent) is Hispanic. Sample members had an average of about two children.

Eight percent of the sample had a high school diploma or GED certificate at program entry (by design, all these individuals were in the Riverside sample), and the average grade completed across the three sites was 10th grade. Twenty-three percent of the sample had recently been enrolled in education or training, and 17 percent were enrolled at program entry.

Achievement tests were administered at the point of random assignment.[31] As the table shows, 63 percent of the sample scored at Level 1 or 2 on the reading test (Level 1 categorizes the lowest test scores; Level 5, the highest), and 66 percent scored at Level 1 or 2 on the math test. The National Adult Literacy Survey, conducted in 1992, found that in the nation as a whole, 72 percent of people on AFDC scored at Levels 1 or 2 on a document literacy test whose scores are analogous to those obtained from the TALS test. Nationwide, 51 percent of all adults scored at Levels 1 or 2 on the national document literacy test.[32]

More than half (58 percent) of sample members in the three sites had ever worked full time for six months or more for one employer, and about one-third (31 percent) had earnings in the year before entering the program. Few sample members were employed when they entered the program. A majority had substantial prior AFDC receipt: 66 percent of the sample had received AFDC for two years or more. About one-third (31 percent) of the sample were living in public, subsidized, emergency, or temporary housing when they entered the study.

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[29] California limited participation in adult education: only those considered to be "in need" of education—those without a high school diploma or GED, those scoring below 215 on the GAIN Appraisal reading or math test, and those who were not proficient in English—could be assigned to education as a first activity.

[30] See Freedman et al., 1999, for the complete sample sizes. Note that California regulations prevented anyone who was not "in need" of education from being randomly assigned to the education-focused program.

[31] In all three sites sample members took the CASAS math test. In Atlanta and Grand Rapids, sample members took the TALS document literacy test. In Riverside, sample members took the state-mandated GAIN Appraisal literacy test, developed by CASAS. Riverside's literacy scores were converted to TALS scores using a "crosswalk" algorithm developed by researchers at Boston College (Haney et al., 1996). See Appendix D for information about the tests.

[32] Kirsch et al., 1993; cited in Pauly, 1995.

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Table 2.2

For Sample Members Without a High School Diploma or GED at Random Assignment: Selected Characteristics of Sample Members, Pooled Across Three Education-Focused Programs

Characteristic	Sample Members
Demographic Characteristics	
Gender (%)	
Male	5.9
Female	94.1
Age (%)	
Less than 19	3.5
19-24	21.0
25-34	46.6
35-44	22.9
45 and over	6.1
Average age (years)	30.8
Ethnicity (%)	
Black	49.1
Hispanic	17.7
White	30.5
Other	2.8
Family Status	
Marital status (%)	
Never married	53.0
Married, living with spouse	4.9
Separated	23.8
Divorced	16.8
Widowed	1.5
Number of children (%)	
1	37.0
2	32.8
3 or more	30.2
Average number of children	2.1
Age of youngest child (%)	
2 or under	18.8
3 to 5	36.3
6 or over	44.9
Education Status	
Received high school diploma or GED certificate (%)	7.5
Average highest grade completed	10.0
Highest grade completed	
8 or below	14.1
9 or 10	38.5
11 or above	47.4

(continued)

Table 2.2 (continued)

Characteristic	Sample Members
Enrolled in education or training in past 12 months (%)	23.3
Currently enrolled in education or training (%)	17.3
Reading and Math Skills	
TALS document literacy test[a]	
Scored at (%)	
Level 1	19.3
Level 2	43.6
Level 3	28.2
Level 4	8.9
Level 5	0.0
Level 1 - 2	62.9
Level 3 - 5	37.1
CASAS math test	
Scored at (%)	
Level 1	18.7
Level 2	47.2
Level 3	21.5
Level 4	12.6
Level 1 - 2	65.9
Level 3 - 4	34.1
Labor Force Status	
Worked full time for 6 months or more for one employer (%)	58.2
Any earnings in past 12 months (%)	30.6
Currently employed (%)	7.1
Public Assistance Status	
Total prior AFDC receipt[b] (%)	
None	0.5
Less than 1 year	21.6
1 year or more but less than 2 years	11.7
2 years or more but less than 5 years	25.7
5 years or more but less than 10 years	18.9
10 years or more	21.6
Raised as a child in a household receiving AFDC (%)	32.2
First spell of AFDC receipt[c] (%)	17.4
Housing Status	
Current housing status (%)	
Public housing	16.0
Subsidized housing	13.3
Emergency or temporary housing	1.5
None of the above	69.3
Sample size	5,863

(continued)

Table 2.2 (continued)

SOURCES: MDRC calculations from information routinely collected by welfare staff and from TALS document literacy test and CASAS math test data.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally in the pooled estimates.

Distributions may not add to 100 percent because of rounding.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

[a] TALS document literacy test scores for Riverside are based on scores earned on the Greater Avenues for Independence (GAIN) Appraisal literacy test and are converted to their TALS equivalent.

[b] This refers to the total number of months accumulated from one or more spells on an individual's own or spouse's case. It does not include AFDC under a parent's name.

[c] This does not mean that such individuals were new to the AFDC rolls, only that this was their first spell on AFDC. This spell, however, may have lasted several years.

VI. Participation in Adult Education

As discussed earlier, this chapter presents program effects from an evaluation that employed a random assignment research design. In such a study, outcomes for control group members represent what would have occurred in the absence of the welfare-to-work intervention being evaluated. In this section, then, participation in employment and training activities by control group members represents the level of participation that occurred without the intervention of the Atlanta, Grand Rapids, and Riverside programs. The difference between the participation level of the program group and the participation level of the control group indicates the *impact* of the programs—the additional participation that the programs induced. Participation estimates presented in this section are based on 2,923 sample members' responses to a survey administered about two years after random assignment.

A. Participation in Program Activities for the Full Sample

As previous NEWWS Evaluation publications have reported, the education-focused programs increased the level of participation in education, training, and employment activities (see Appendix Table C.2 for participation results broken down by site).[33] As Table 2.3 shows (pooling data across the three programs), 28 percent of the control group participated, for at least one day, in at least one employment or training activity within two years of entering the program. In other words, without the intervention of the welfare-to-work programs, 28 percent of welfare recipients took part in employment-related activities. The programs substantially increased this level of participation: 63 percent of the program group participated—an impact of 35 percentage points.[34]

Reflecting the programs' focus on education, the largest program effects presented in Table 2.3 were on education and training activities. Owing to the low educational attainment and achievement of the nongraduate sample at program entry, the largest increases were in adult education: 50 percent of the program group participated in adult education—a 33 percentage point impact above the control group participation rate of 18 percent. This impact reflects a 30 percentage point increase in participation in ABE or GED classes (the survey did not distinguish between these two types of activities) and a 2 percentage point increase in participation in both ESL and high school completion classes. The 33 percentage point increase in adult education participation is high in the range of impacts found previously for mandatory welfare-to-work programs.[35]

The goal of these programs was not simply to augment basic skills but also to move participants into the labor market. Reflecting this goal, as Table 2.3 presents, the programs increased participation in job search (by 14 percentage points). Notice that the programs increased the pro-

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[33] See Hamilton et al., 1997, for detailed participation results for the education- and employment-focused programs in Atlanta, Grand Rapids, and Riverside; see Freedman et al., 1999, for participation results for all 11 programs in the NEWWS Evaluation.

[34] All impact estimates presented in this chapter are regression-adjusted, controlling for pre-random assignment characteristics of sample members.

[35] Impacts on participation in adult education in mandatory programs discussed in Pauly, 1995, ranged from 4 to 37 percentage points.

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Table 2.3

For Sample Members Without a High School Diploma or GED at Random Assignment: Two-Year Impacts on Participation in Program Activities, Pooled Across Three Education-Focused Programs

	Participated (%)			Hours of Participation			*Hours of Participation Among Participants*		
Activity	Program Group	Control Group	Difference (Impact)	Program Group	Control Group	Difference (Impact)	*Program Group*	*Control Group*	*Difference (Impact)*
Any activity	63.3	28.0	35.3 ***	335.7	124.6	211.1 ***	*530.0*	*444.2*	*85.8*
Job search	19.1	5.3	13.8 ***	24.6	4.9	19.6 ***	*128.8*	*93.4*	*35.5*
Education or training activity	56.5	24.9	31.6 ***	311.1	119.6	191.5 ***	*550.8*	*480.1*	*70.7*
Adult education	50.1	17.6	32.5 ***	244.4	68.2	176.2 ***	*488.1*	*387.6*	*100.4*
ABE or GED	42.6	13.0	29.6 ***	189.4	41.8	147.6 ***	*444.3*	*321.6*	*122.6*
ESL	3.5	1.5	2.0 ***	20.3	7.5	12.8 ***	*587.9*	*506.9*	*81.0*
High school completion[a]	6.1	4.1	1.9 **	34.7	18.8	15.9 ***	*571.0*	*454.6*	*116.4*
College	4.5	2.8	1.7 **	23.0	14.2	8.8 *	*512.6*	*516.3*	*-3.7*
Vocational training	9.4	7.4	2.0 *	43.7	37.3	6.4	*463.6*	*500.5*	*-36.8*
Work experience or on-the-job training	2.0	1.1	0.8 *	N/a	N/a		*N/a*	*N/a*	
Adult education *and* job search	11.7	2.0	9.7 ***	67.9	10.7	57.2 ***	*581.7*	*532.1*	*49.6*
Adult education *and* college or vocational training	7.2	2.5	4.8 ***	65.7	20.8	44.9 ***	*908.3*	*844.1*	*64.2*
Sample size	1,415	1,508		1,415	1,508		*(varies)*	*(varies)*	

(continued)

Table 2.3 (continued)

SOURCE: MDRC calculations from the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Survey respondents were interviewed between month 25 and month 31, counting the month in which random assignment occurred as month 1.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally in the pooled estimates.

Estimates were regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members.

Numbers may not add up to 100 percent because of rounding.

Sample sizes for individual measures vary because of missing values.

A two-tailed t-test was applied to differences between outcomes for the program and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

Italics are used to signal average outcomes and differences that were calculated only for participants. Unlike the full-sample program and control groups, these program and control groups may differ from each other in average background characteristics. Such differences could have influenced the types of employment-related activities people in the two groups attended or their length of stay. If so, the program-control differences might understate or overstate the effects of the programs. Because these impact estimates are less reliable than those based on the full sample, statistical significance tests of these results were not conducted.

N/a = not available or applicable.

[a] This may include regular high school classes.

portion of those who participated in both adult education *and* job search (by 10 percentage points) and in both adult education *and* college or vocational training (by 5 percentage points). Few participated in these combinations of activities without the intervention of the welfare-to-work programs (indicated by the control group participation levels—2 and 3 percent, respectively).

In addition, the programs increased the length of time spent in activities. Focusing on adult education, notice that program group members spent an average of 176 more hours in this activity than their control group counterparts. (This experimental comparison includes *all* program and control group members who were surveyed; zero hours are counted for each of those who did not participate in adult education.) Presented in terms of months (not shown in the table), this means that control group members participated in adult education for an average of 1.2 months, while program group members participated for an average of 3.7 months—an increase of 2.5 months.

Considering only those who participated in adult education (a nonexperimental comparison, because program group participants may have different characteristics than their control group counterparts), it is apparent that participating program group members spent substantially more hours in class than did control group members who were surveyed; an average of 488 hours compared with 388 hours, respectively. Program group participants spent 123 more hours in ABE or GED class, 81 more hours in ESL class, and 116 more hours in high school completion classes.

As mentioned, these welfare-to-work programs were mandatory; welfare recipients could lose part of their grant if they failed to comply with program requirements. Across the three programs, about one-quarter (28 percent) of program group members were sanctioned at some point during the two years following their entry into the programs.[36] The three programs have been characterized in earlier NEWWS Evaluation reports as strongly enforcing the participation mandate.[37]

B. Participation in Adult Education for Various Subgroups

The preceding findings establish the ability of these education-focused programs to increase participation in adult education activities. However, it is useful to know whether these programs were able to do so for a range of different subgroups, including groups with low levels of education, high levels of barriers to participation, and little motivation to participate. This section examines participation in adult education for a number of interesting subgroups.

Program effects were estimated for groups defined using the following baseline characteristics (that is, characteristics at the time of random assignment):

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[36] The sanctioning rates differed by program. In Atlanta, 26 percent were sanctioned; in Grand Rapids, 39 percent were sanctioned; and in Riverside, 21 percent were sanctioned. See Hamilton et al., 1997, for a complete presentation of sanctioning rates and a discussion of enforcement in the three programs. (The rates presented here are based on client survey data. Rates presented in Hamilton et al., 1997, are based on case file data.)

[37] See Freedman et al., 1999.

- education levels (TALS document literacy test and CASAS math test scores and highest grade completed)
- level of economic disadvantage (based on work and welfare history)
- age of youngest child
- self-reported family or personal problems
- self-reported parental concerns (concerns about leaving family for work or school)
- self-reported preference for school
- self-reported depressive symptoms

The last four subgroups were created using data from a self-administered questionnaire, completed when sample members entered the study.[38] The creation of these subgroups is described in Appendix E.[39]

As Table 2.4 shows, the programs produced substantial impacts on both the level and the length of participation in adult education for each of the 20 subgroups examined. Participation in adult education was increased by 25 to 36 percentage points, and hours spent in adult education were increased by 74 to 237 hours. This shows that mandatory welfare-to-work programs can successfully increase participation in adult education for many different types of clients, including those facing barriers to participation or those unmotivated to enroll.

As the first rows of the table show, control group members with low scores (Level 1-2) on the reading test were less likely to seek out adult education programs on their own than control group members with high scores (Level 3-5). Nonetheless, the program effects on participation in adult education were remarkably similar for the two subgroups (33 percentage points for those with low scores and 31 percentage points for those with high scores). This is also true for the subgroups defined using math test scores.

Program effects on hours of participation in adult education were substantial for all four test-score subgroups but tended to be larger for those with low scores. The programs increased adult education participation by 194 hours for those with low reading scores, compared with an increase of 139 hours for those with high reading scores. (The "daggers" shown in the table indicate whether subgroup effects were statistically significantly different from one another.) Focusing only on participants in adult education among these subgroups (the last set of columns), note that the programs produced larger increases in hours of adult education for those with low reading and low math scores than for those with high scores.

Impacts on the level and hours of participation in adult education were similar across the subgroups defined by highest grade completed before entering the program. Among only those

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[38] See Appendix C in Hamilton and Brock, 1994, for a copy of the Private Opinion Survey instrument.

[39] Sample sizes vary across subgroups because some sample members are missing the relevant baseline or Private Opinion Survey measures.

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Table 2.4

For Sample Members Without a High School Diploma or GED at Random Assignment: Two-Year Impacts on Participation in Adult Education, Pooled Across Three Education-Focused Programs, by Selected Characteristics at the Time of Random Assignment

		Participated in Adult Education (%)			Hours of Participation in Adult Education			*Hours of Participation in Adult Education Among Participants*		
Subgroup	Sample Size	Program Group	Control Group	Difference (Impact)	Program Group	Control Group	Difference (Impact)	*Program Group*	*Control Group*	*Difference (Impact)*
Score on TALS document literacy test[a]							††			
Level 1 - 2	1,731	47.9	14.7	33.2 ***	252.3	58.8	193.5 ***	*526.4*	*399.3*	*127.1*
Level 3 - 5	980	53.1	22.2	30.9 ***	226.3	87.4	138.8 ***	*425.8*	*394.0*	*31.7*
Score on CASAS math test										
Level 1 - 2	1,864	46.7	14.5	32.3 ***	239.9	52.0	187.9 ***	*513.6*	*360.1*	*153.5*
Level 3 - 4	846	56.4	23.3	33.1 ***	250.2	102.2	148.1 ***	*443.8*	*439.2*	*4.6*
Highest grade completed in school										
8 or below	409	51.9	20.7	31.1 ***	266.8	121.4	145.3 ***	*514.4*	*585.5*	*-71.0*
9 - 10	1,065	55.1	18.8	36.3 ***	280.2	75.0	205.2 ***	*508.5*	*398.6*	*109.9*
11 or above	1,432	45.8	15.5	30.2 ***	210.3	46.7	163.6 ***	*459.7*	*301.0*	*158.7*
Level of economic disadvantage[b]				††			†††			
More disadvantaged	1,454	51.5	15.9	35.6 ***	281.8	63.4	218.4 ***	*547.1*	*397.6*	*149.5*
Less disadvantaged	1,454	48.3	19.5	28.8 ***	206.5	74.7	131.8 ***	*427.5*	*382.8*	*44.6*
Age of youngest child										
5 or under	1,305	53.7	22.3	31.3 ***	274.0	78.5	195.5 ***	*510.5*	*351.3*	*159.3*
6 or over	1,618	46.5	12.7	33.8 ***	215.3	56.5	158.8 ***	*463.5*	*445.5*	*18.1*

(continued)

Table 2.4 (continued)

Subgroup	Sample Size	Participated in Adult Education (%)			Hours of Participation in Adult Education			*Hours of Participation in Adult Education Among Participants*		
		Program Group	Control Group	Difference (Impact)	Program Group	Control Group	Difference (Impact)	*Program Group*	*Control Group*	*Difference (Impact)*
Reported barriers to participation[c]										
Many family or personal problems *and* parental concerns	327	43.2	16.7	26.4 ***	198.9	27.4	171.5 ***	*460.7*	*163.8*	*297.0*
Many family or personal problems	223	40.3	15.3	24.9 ***	190.2	78.6	111.5 **	*472.3*	*512.6*	*-40.3*
Many parental concerns	348	52.8	18.0	34.8 ***	295.2	57.8	237.4 ***	*558.9*	*321.7*	*237.2*
Neither barrier	1,358	53.2	18.5	34.7 ***	259.7	85.9	173.8 ***	*487.9*	*464.0*	*23.9*
Preference for school[c]										
Do not like and/or do not plan to attend school	873	43.7	8.9	34.8 ***	218.3	18.6	199.7 ***	*499.2*	*208.9*	*290.4*
Like and/or plan to attend school	1,504	53.8	23.4	30.5 ***	274.0	103.6	170.4 ***	*508.9*	*443.3*	*65.6*
Depressive symptoms[c]							†††			
Many symptoms	346	46.2	20.1	26.0 ***	190.3	115.9	74.4 **	*412.1*	*575.0*	*-162.9*
Moderate number of symptoms	592	50.2	15.8	34.4 ***	261.9	42.7	219.2 ***	*521.5*	*270.0*	*251.5*
Few symptoms	1,275	51.8	19.1	32.7 ***	253.9	77.7	176.2 ***	*489.8*	*406.2*	*83.6*

(continued)

Table 2.4 (continued)

SOURCES: MDRC calculations from information routinely collected by welfare staff, TALS document literacy test and CASAS math test data, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Adult education includes ABE, GED, ESL, and high school completion classes.

Survey respondents were interviewed between month 25 and month 31, counting the month in which random assignment occurred as month 1.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally in the pooled estimates.

Estimates were regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members.

Sample sizes for individual measures vary because of missing values.

Rounding may cause slight discrepancies in calculating sums and differences.

A two-tailed t-test was applied to differences between outcomes for the program and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

An F-test was applied to differences among subgroups for each characteristic. Statistical significance levels are indicated as: † = 10 percent; †† = 5 percent; and ††† = 1 percent.

Italics are used to signal average outcomes and differences that were calculated only for adult education participants. Unlike the full-sample program and control groups, these program and control groups may differ from each other in average background characteristics. Such differences could have influenced the types of employment-related activities people in the two groups attended or their length of stay. If so, the program-control differences might understate or overstate the effects of the programs. Because these impact estimates are less reliable than those based on the full sample, statistical significance tests of these results were not conducted.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

[a]TALS scores for Riverside are based on scores earned on the Greater Avenues for Independence (GAIN) Appraisal literacy test and are converted to their TALS equivalent.

[b]"More disadvantaged" individuals are those who did not work for pay in the year prior to random assignment and who received AFDC for more than two years prior to random assignment. The "less disadvantaged" category contains those who did not meet these criteria.

[c]These subgroups are based on scales created from Private Opinion Survey data. For an explanation of these subgroups see discussion in Appendix E.

who participated in adult education, the programs actually *decreased* the hours of participation for those entering at an eighth grade level or below. This probably results from three main factors. First, control group members in this subgroup who participated in adult education participated for more hours (586 hours) than any other control group members, so the programs had a high "threshold" to surmount. Second, the welfare-to-work programs generally limited the duration of participation in education (6 to 12 months in Riverside, two years in Atlanta and Grand Rapids), and they possibly diverted some clients who would have continued in education into another activity, such as job search or vocational training. Third, many clients whom the programs involved in adult education would have participated for *zero* hours without the program intervention; these individuals may have had substantial participation, even if it was less than the control group average of 586 hours.

The programs produced larger impacts on participation for the economically more disadvantaged subgroup than for the less disadvantaged subgroup. Fifty-two percent of the more disadvantaged program group members participated in adult education—a 36 percentage point increase above the control group level. The corresponding increase for the less disadvantaged was only 29 percent. Likewise, the difference in adult education participation for the more disadvantaged was 218 hours, compared with a 132-hour increase for the less disadvantaged.

As Table 2.4 shows, the impacts for subgroups defined using reported barriers to participation were statistically indistinguishable. However, the 25 percentage point impact for the subgroup with many family or personal problems is the smallest program effect among all 20 subgroups. The impact for those who reported having many concerns about leaving their families to go to work or to participate in a job search or education activity (many parental concerns) was 35 percentage points—one of the largest impacts among all the subgroups, and identical to the impact for the subgroup with neither barrier (35 percentage points). The hours impact for this subgroup (237 hours) is the largest among all 20 subgroups. This suggests that the questions asked to create the parental concerns scale do not capture a true barrier to participation but simply a preference (see Appendix E for the questions included in the scale). Ideally, perhaps, these persons would remain at home with their families, but in the context of the 1990s welfare and economic environment, they are able to attend school.

The subgroup who said that they like school and/or plan to attend school can be considered more motivated to participate in an education activity than those who did not. One would expect, then, that the adult education participation level of control group members who expressed a preference for school would be higher; this is true (23 percent, compared with 9 percent for those who did not express such a preference). It would not be surprising for mandatory welfare-to-work programs—given their incentives and penalties—to further increase education participation for a motivated subgroup, and this was indeed the case. It would also be reasonable to assume that programs would have more trouble engaging people in education activities if they did not express a preference for school. Unexpectedly, though, the programs studied were also successful in increasing adult education participation for this subgroup—by 35 percentage points and by 200 hours. Program group members in this subgroup who participated in adult education stayed in the activity for about as long as program group members who expressed a preference for school (499 hours, compared with 509 hours).

For persons with many symptoms of depression, the programs produced smaller participation increases than for those with a moderate number of symptoms or few symptoms: the 26 percentage point impact on level of participation is smaller than the impacts for the other two subgroups (although the difference is not statistically significant), and the 74-hour impact is much smaller than the impacts on hours for the other subgroups (and is the smallest hours impact among all the subgroups). The magnitude of this impact is a product of somewhat short stays in education for the program group and long stays for the control group. In fact, among participants, the programs *decreased* the length of stay in adult education for this subgroup by 163 hours. This decrease may reflect that some members of the program group left welfare and thus stopped attending class, that the programs limited the length of stay in education and removed those who were not making progress, or that the programs provided supports (perhaps mental health counseling) that allowed students to progress more rapidly through class. It is also possible that the programs "exempted" some clients from the participation requirement because of their mental condition.

VII. Effects of the Education-Focused Programs on Key Educational Outcomes

The education-focused programs in Atlanta, Grand Rapids, and Riverside substantially increased participation in adult education. This section discusses whether the programs also increased educational attainment (receipt of education credentials) and educational achievement (reading and math skills). The impacts examined in this section are based on comparisons between average outcomes for the control group and the program group. The averages include outcomes both for individuals who participated in adult education and for individuals who did not. Thus, any effects of the education itself may be understated. Also, although treatment effects could have been caused by other program components that were unrelated to adult education, if a program improved educational outcomes—for example, by increasing receipt of a GED—it is unlikely that these impacts were caused by the program's job search or training activities.

A. Impacts on Educational Attainment

1. Educational Attainment for the Full Sample. Table 2.5 presents the programs' impacts on educational attainment using two-year survey data for 2,923 sample members. As the table's first row shows, few control group members (4 percent) received a high school diploma or GED during the two years following random assignment. Eleven percent of the program group received such a credential, for a modest impact of 7 percentage points.[40] This program effect mostly reflects receipt of GED certificates, and its magnitude is in the range of impacts found in previous evaluations.[41] None of the three programs studied here increased receipt of a training

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[40] Impacts varied across the three sites: the Atlanta program did not produce an impact on high school diploma or GED receipt; the Grand Rapids program produced an 11 percentage point impact; and the Riverside program produced an 8 percentage point impact (see Freedman et al., 1999).

[41] Fourteen of 17 programs reviewed in Pauly, 1995, produced high school diploma or GED attainment impacts, ranging from 2 to 19 percentage points.

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Table 2.5

For Sample Members Without a High School Diploma or GED at Random Assignment: Impacts on Receipt of a High School Diploma or GED at the End of Two Years, Pooled Across Three Education-Focused Programs, for the Full Sample and by Selected Characteristics at the Time of Random Assignment

			Received a High School Diploma or GED (%)			
Sample or Subgroup	Sample Size	Impact on Hours of Participation in Adult Education	Program Group	Control Group	Difference (Impact)	Effect Size[a]
Full sample	2,923	176.2	10.6	3.6	7.1 ***	0.38
Score on TALS document literacy test[b]					†††	
Level 1 - 2	1,731	193.5	4.3	1.1	3.1 ***	0.17
Level 3 - 5	980	138.8	23.4	7.5	15.9 ***	0.86
Score on CASAS math test					†††	
Level 1 - 2	1,864	187.9	3.8	1.0	2.8 ***	0.15
Level 3 - 4	846	148.1	26.3	8.6	17.8 ***	0.96
Highest grade completed in school					†††	
8 or below	409	145.3	3.5	1.2	2.3	0.13
9 - 10	1,065	205.2	11.2	2.9	8.3 ***	0.45
11 or above	1,432	163.6	12.5	4.7	7.8 ***	0.42
Level of economic disadvantage[c]						
More disadvantaged	1,454	218.4	7.9	2.0	5.8 ***	0.32
Less disadvantaged	1,454	131.8	13.3	5.0	8.3 ***	0.45
Age of youngest child						
5 or under	1,305	195.5	14.1	5.3	8.8 ***	0.48
6 or over	1,618	158.8	7.2	1.7	5.5 ***	0.30
Reported barriers to participation[d]						
Many family or personal problems *and* parental concerns	327	171.5	4.2	2.6	††† 1.7	0.09
Many family or personal problems	223	111.5	5.9	2.9	3.0	0.16
Many parental concerns	348	237.4	18.0	3.6	14.4 ***	0.78
Neither barrier	1,358	173.8	11.7	4.3	7.4 ***	0.40
Preference for school[d]						
Do not like and/or do not plan to attend school	873	199.7	8.9	1.8	7.1 ***	0.39
Like and/or plan to attend school	1,504	170.4	11.8	4.9	6.8 ***	0.37
Depressive symptoms[d]						
Many symptoms	346	74.4	14.8	5.2	9.6 ***	0.52
Moderate number of symptoms	592	219.2	11.6	3.3	8.3 ***	0.45
Few symptoms	1,275	176.2	10.8	3.8	7.0 ***	0.38

(continued)

Table 2.5 (continued)

SOURCES: MDRC calculations from information routinely collected by welfare staff, TALS document literacy test and CASAS math test data, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Adult education includes ABE, GED, ESL, and high school completion classes.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

Survey respondents were interviewed between month 25 and month 31, counting the month in which random assignment occurred as month 1.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally in the pooled estimates.

Estimates were regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members.

Sample sizes for individual measures vary because of missing values.

Rounding may cause slight discrepancies in calculating sums and differences.

A two-tailed t-test was applied to differences between outcomes for the program and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

An F-test was applied to differences among subgroups for each characteristic. Statistical significance levels are indicated as: † = 10 percent; †† = 5 percent; and ††† = 1 percent.

[a] The effect size equals the difference (impact) divided by the standard deviation of the outcome measure for the full sample control group.

[b] TALS scores for Riverside are based on scores earned on the Greater Avenues for Independence (GAIN) Appraisal literacy test and are converted to their TALS equivalent.

[c] "More disadvantaged" individuals are those who did not work for pay in the year prior to random assignment and who received AFDC for more than two years prior to random assignment. The "less disadvantaged" category contains those who did not meet these criteria.

[d] These subgroups are based on scales created from Private Opinion Survey data. For an explanation of these subgroups see Appendix E.

certificate or trade license (for those entering the program without a high school diploma or GED).[42]

2. Educational Attainment for Various Subgroups. As Table 2.5 shows, impacts on receipt of a high school diploma or GED certificate were produced for most, but not all, subgroups. As has been found in previous studies, impacts were much larger for persons entering the program at higher educational achievement levels. The few subgroups without impacts were those who had dropped out of school very early (before beginning ninth grade) and those with many family or personal problems. There appears to be no clear relationship between the impact on hours of participation in adult education and impacts on educational attainment: subgroups with larger increases in the number of hours do not necessarily have larger high school diploma or GED impacts. (As mentioned, the relationship between length of participation and educational attainment is explored more extensively in Chapter 3.)

Twenty-three percent of program group members who had high scores on the reading test at program entry received a diploma or GED within two years. This represents a 16 percentage point impact above the control mean of 8 percent. The corresponding impact for the subgroup with high math scores is 18 percentage points.

Each percentage point impact on Table 2.5 is expressed also as an *effect size*, which equals the difference (impact) divided by the standard deviation of the outcome measure for the control group.[43] This transformation of the impacts standardizes the results for comparison across subgroups and across different studies and for comparison to the results presented in the next section, on educational achievement.

As the table's last column shows, the impacts on high school diploma or GED attainment for the two subgroups with high test scores were large: .86 of a standard deviation for the high reading score group and .96 of a standard deviation for the high math score group. These impacts are the largest of all the subgroups examined (effect sizes for the other statistically significant impacts range from .15 to .78 of a standard deviation).

For the subgroups defined by the highest grade completed in school, impacts on receipt of a high school diploma or GED were found only for sample members who had previously completed some high school, whether an earlier grade (9 or 10) or a later grade (11 or above). The subgroup that had completed eighth grade or below had no attainment impact (the 2 percentage point impact is not statistically significant).

The programs did not increase receipt of a high school diploma or GED for those who reported many family or personal problems at program entry. (See the results for the first two subgroups listed under reported barriers to participation. The first includes people with both many family or personal problems and parental concerns, and the second includes those who have many family or personal problems but not many parental concerns.) Interestingly, program effects among subgroups defined using family or personal barriers were largest for those who reported many parental concerns. Again, as suggested in the previous section, this indicates that the scale is not capturing permanent barriers to successful program participation.

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[42] See Freedman et al., 1999, for a complete presentation of education and training credential results for these programs and the other NEWWS Evaluation programs.

[43] The standard deviation for the entire control group (without a high school diploma or GED) was used to calculate each subgroup's effect size.

Notably, reflecting the adult education participation findings, the programs produced impacts on high school diploma or GED attainment for people who said they did not like school or did not plan to go to school. The 7 percentage point impact for this subgroup represents .39 of a standard deviation.

Despite the substantial variation in impacts on hours of participation across the three subgroups defined in terms of depressive symptoms, impacts on receipt of a high school diploma or GED were very similar. Most notably, for those with many symptoms of depression, the somewhat small 74-hour increase in adult education participation was accompanied by a 10 percentage point impact on GED receipt. Not shown in the table, the programs also produced a small impact on the receipt of a trade license or certificate for the subgroup with many symptoms of depression.[44] This shows that some combination of the programs' supports, incentives, and mandates helped students complete education and training.

It is important to keep in mind that these findings represent only two years of follow-up. In some cases—such as for those entering with low educational attainment—gains in educational attainment might be delayed as persons continue participating in education or improve their skills through work. The final report in the NEWWS Evaluation will present five-year educational attainment results for these three programs.

As previously mentioned, the welfare-to-work programs studied here emphasized education and training, based on the belief that an up-front investment in the skills levels of welfare recipients allows them to obtain higher-paying and more secure jobs. As was just presented, the programs increased receipt of GED certificates; Chapter 4 examines whether receipt of a GED certificate led to higher earnings.

B. Impacts on Educational Achievement

The primary measure of educational achievement in the NEWWS Evaluation in these three sites is performance on the Test of Applied Literacy Skills (TALS) document literacy test, which was developed by the Educational Testing Service. (See Appendix D for a more detailed description of this test.) About two years after random assignment the test was administered to program and control group members who did not have a high school diploma or GED at the time of random assignment[45] (and to those in Riverside who had a diploma or GED but had low reading or math skills).[46]

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[44] Nine percent of program group members in this subgroup received a trade license or certificate, compared with 4 percent of control group members—a 5 percentage point impact. The only other subgroup with an impact on the receipt of a trade license or certificate is the more disadvantaged subgroup, with a 3 percentage point impact.

[45] Administration of an achievement test as part of a survey conducted in sample members' homes poses some concerns. Because the outcome of such a test has no bearing on sample members' lives and because of distractions (from family members, visitors, etc.), the test's reliability will be less than that of a test administered in a school.

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Reading test scores were analyzed for sample members for whom both baseline and follow-up test score data were available (2,532 sample members). Impacts are stated both as absolute numerical effects and as effect sizes (the impact divided by the standard deviation of the test score calculated for all control group members in the research sample).[47] Beyond the advantages discussed in the previous section, stating impacts as an effect size is particularly useful when discussing test scores because it provides a reference for judging absolute impacts large or small. For example, a test score impact of 25 would be considered trivial if it were .02 of a standard deviation but would be meaningful if it were .25 of a standard deviation.

In summary, small impacts on reading test scores were found for the subgroup whose youngest child was 6 or older at the time of random assignment, but no evidence of reading score impacts was found for the sample as a whole or for any other subgroup. Subgroups for which increases in adult education hours were relatively large did not necessarily show subsequent impacts on reading scores.

1. Reading Scores for the Full Sample. As shown in Table 2.6, the programs did not produce an impact on sample members' average reading score. The programs, however, did raise a small proportion from Level 2 to Level 3 (accompanied by statistically insignificant reductions in the number of sample members scoring in Levels 4 and 5). According to test developers, the upward movement represents an improvement from having a severe labor market handicap to a level possessed by many employees in service, clerical, and other low-skill occupations. (See Appendix Table C.3 for program effects broken down by site.)

2. Reading Scores for Various Subgroups. Table 2.7 presents impacts of the three programs on the number of hours of adult education and reading scores for several subgroups of the sample of test-takers. Impacts on reading scores were found only for the subgroup of sample members whose youngest child was 6 or older at the time of random assignment, although the impact on hours of participation in adult education for these clients was not different from that for those with younger children.[48] This impact represents a gain of only .07 of a standard deviation—a small impact. None of the other subgroup impact estimates was statistically significantly different from zero. Except for the age of youngest child subgroup, the variation in reading score impacts across other subgroups was not statistically significant. There appears to be little correlation between impacts on average hours of participation and test scores.

Table 2.8 presents detailed achievement impacts for those whose youngest child was 6 or older at the time of random assignment. This category of sample members was also administered

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While this may not directly affect the difference in test scores between program group members and control group members, it might make it more difficult to find statistically significant program effects.

[46] Recall that Riverside's sample for this chapter includes people who were determined to be "in need" of education at random assignment (discussed in section V of this chapter). Across all sites, those with limited reading skills did not take the reading test and thus are not included in the educational achievement analysis.

[47] All test score impacts presented in this section are regression-adjusted, controlling for pre-random assignment characteristics and baseline test scores of sample members.

[48] When using a p-value of .1 as the cutoff to identify statistically significant impacts, one might expect to find an apparent impact for 1 of every 10 subgroups even if none of the groups really experienced one. Thus, this impact may simply represent random variation.

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Table 2.6

For Sample Members Without a High School Diploma or GED at Random Assignment: Impacts on TALS Document Literacy Test Scores at the End of Two Years, Pooled Across Three Education-Focused Programs

Measure	Program Group	Control Group	Difference (Impact)	Effect Size[a]
Average TALS document literacy test score[b]	250	249	0.5	0.01
Scored at (%)				
Level 1	26.2	24.9	1.2	0.03
Level 2	41.2	44.7	-3.4 *	-0.07
Level 3	26.9	23.7	3.3 **	0.08
Level 4	5.7	6.6	-1.0	-0.04
Level 5	0.0	0.1	-0.1	-0.03
Sample size	1,230	1,302		

SOURCE: MDRC calculations from TALS document literacy test data.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally in the pooled estimates.

Estimates were regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics and baseline test scores of sample members.

Rounding may cause slight discrepancies in calculating sums and differences.

A two-tailed t-test was applied to differences between program and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

[a] The effect size equals the difference (impact) divided by the standard deviation of the outcome measure for the full sample control group.

[b] TALS = Test of Applied Literacy Skills.

Table 2.7

For Sample Members Without a High School Diploma or GED at Random Assignment: Impacts on TALS Document Literacy Test Scores at the End of Two Years, Pooled Across Three Education-Focused Programs, by Selected Characteristics at the Time of Random Assignment

			TALS Document Literacy Test Score			
Subgroup	Sample Size	Impact on Hours of Participation in Adult Education	Program Group	Control Group	Difference (Impact)	Effect Size[a]
Score on TALS document literacy test[b]						
Level 1 - 2	1,593	193.5	230	228	1.5	0.03
Level 3 - 5	939	138.8	282	284	-1.3	-0.02
Score on CASAS math test						
Level 1 - 2	1,720	187.9	234	232	2.1	0.04
Level 3 - 4	812	148.1	279	282	-2.4	-0.05
Highest grade completed in school						
8 or below	237	145.3	233	227	5.4	0.10
9 - 10	978	205.2	247	248	-1.0	-0.02
11 or above	1,317	163.6	256	255	0.4	0.01
Level of economic disadvantage[c]						
More disadvantaged	1,294	218.4	242	242	-0.7	-0.01
Less disadvantaged	1,226	131.8	258	256	1.6	0.03
Age of youngest child					††	
5 or under	1,155	195.5	257	260	-2.8	-0.05
6 or over	1,377	158.8	243	239	3.9 *	0.07
Reported barriers to participation[d]						
Many family or personal problems *and* parental concerns	289	0.0	232	234	-2.0	-0.04
Many family or personal problems	201	171.5	238	242	-4.3	-0.08
Many parental concerns	316	111.5	256	255	1.7	0.03
Neither barrier	1,260	237.4	257	254	2.8	0.05
Preference for school[d]						
Do not like and/or do not plan to attend school	798	0.0	245	243	1.9	0.02
Like and/or plan to attend school	1,363	199.7	254	253	0.7	0.01
Depressive symptoms[d]						
Many symptoms	321	0.0	248	248	0.4	0.01
Moderate number of symptoms	543	74.4	252	253	-1.0	-0.02
Few symptoms	1,161	219.2	253	252	0.6	0.01

(continued)

Table 2.7 (continued)

SOURCES: MDRC calculations from information routinely collected by welfare staff, TALS document literacy test and CASAS math test data, and Private Opinion Survey data. Calculation of impacts on TALS scores are based on all sample members with baseline and follow-up test score data.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Adult education includes ABE, GED, ESL, and high school completion classes.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally in the pooled estimates.

Estimates were regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics and baseline test scores of sample members.

Sample sizes for individual measures vary due to missing values.

Rounding may cause slight discrepancies in calculating sums and differences.

A two-tailed t-test was applied to differences between outcomes for the program and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

An F-test was applied to differences among subgroups for each characteristic. Statistical significance levels are indicated as: † = 10 percent; †† = 5 percent; and ††† = 1 percent.

[a] The effect size equals the difference (impact) divided by the standard deviation of the outcome measure for the full sample control group.

[b] TALS scores for Riverside are based on scores earned on the Greater Avenues for Independence (GAIN) Appraisal literacy test and are converted to their TALS equivalent.

[c] "More disadvantaged" individuals are those who did not work for pay in the year prior to random assignment and who received AFDC for more than two years prior to random assignment. The "less disadvantaged" category contains those who did not meet these criteria.

[d] These subgroups are based on scales created from Private Opinion Survey data. For an explanation of these subgroups see discussion in Appendix E.

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Table 2.8

For Sample Members Without a High School Diploma or GED, Whose Youngest Child Was 6 or Older at the Time of Random Assignment: Impacts on TALS Document Literacy Test and CASAS Math Test Scores at the End of Two Years, Pooled Across Three Education-Focused Programs

Measure	Program Group	Control Group	Difference (Impact)	Effect Size[a]
TALS document literacy and CASAS math tests				
Scored at Level 3 or above on literacy *or* math test (%)	41.7	36.4	5.3 **	0.12
Scored at Level 3 or above on literacy *and* math tests (%)	18.7	13.8	5.0 ***	0.15
TALS document literacy test				
Average score	243	239	3.5 *	0.07
Scored at (%)				
Level 1	31.6	32.6	-1.0	-0.02
Level 2	40.7	45.0	-4.4	-0.09
Level 3	24.2	17.9	6.3 ***	0.17
Level 4	3.5	4.4	-0.9	-0.05
Level 5	0.0	0.0	0.0	N/a
Level 1 - 2	72.3	77.7	-5.3 **	-0.13
Level 3 - 5	27.7	22.3	5.3 **	0.13
CASAS math test				
Average score	207	206	0.9	0.06
Scored at (%)				
Level 1	24.0	25.3	-1.4	-0.03
Level 2	43.3	46.9	-3.6	-0.08
Level 3	23.0	17.5	5.5	0.15
Level 4	9.7	10.3	-0.5	-0.02
Level 1 - 2	67.2	72.2	-5.0 **	-0.12
Level 3 - 4	32.8	27.8	5.0 **	0.12
Sample size	662	664		

(continued)

Table 2.8 (continued)

SOURCES: MDRC calculations from TALS document literacy test and CASAS math test data.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally in the pooled estimates.

Estimates were regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics and baseline test scores of sample members.

Rounding may cause slight discrepancies in calculating sums and differences.

A two-tailed t-test was applied to differences between program and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

N/a = not available or applicable.

[a] The effect size equals the difference (impact) divided by the standard deviation of the outcome measure for the full sample control group.

the CASAS math test.[49] As shown in the table and as mentioned above, there was a small impact on average reading scores. Impacts for this subgroup were found mostly in Atlanta (not shown in the table). There was no impact on average math scores; however, there was a 6 percentage point increase in the number of program group members scoring at Level 3 on the math test (not statistically significant).[50] Overall, for those whose youngest child was 6 or older, there was a 5 percentage point increase in the number of program group members scoring at Level 3 or above on both the reading and math tests. This represents a gain of .15 of a standard deviation.[51]

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[49] The sample presented here is slightly different from that presented in Table 2.7; Table 2.8 includes only those who took both the TALS document literacy test and the CASAS math test.

[50] Appendix Table D.2 describes the skills associated with the different scoring levels for the CASAS math test.

[51] CASAS math results are also available for 120 sample members in Grand Rapids whose youngest child was 1 to 3 years old at the time of random assignment. There was no statistically significant impact on the TALS literacy or CASAS math scores for this group. However, the program did produce a 13 percentage point increase in the number of program group members scoring at Level 4 on the CASAS math test (not shown in any table).

Chapter 3

Individual Efforts and School Effects: The Payoff to Participation in Adult Education

I. Introduction

A key purpose of this report is to explore and validate the model underlying education-focused, or Human Capital Development (HCD) programs, which emphasize skill-building through education as a precursor to employment. In particular, the report attempts to test the hypothesis that an initial investment in human capital will result in increases in welfare recipients' skills and educational attainment, which in turn will improve both their ability to secure employment and the wage at which they are employed. Chapter 2 began to explore this issue in three sites (Atlanta, Georgia; Grand Rapids, Michigan; and Riverside, California) which operated education-focused programs alongside more employment-focused, or Labor Force Attachment (LFA), programs. It described the characteristics of the adult education providers through which welfare recipients received adult education services, and it explored the effects of education-focused welfare-to-work programs on participation in these adult education activities.

Among other things, the analysis in Chapter 2 indicated that mandatory participation in education-focused programs increased the amount of time spent in adult education for many different groups of welfare recipients. This chapter examines the relationship between the time spent in adult education and welfare recipients' educational achievement and attainment. Within the three sites that operated both employment- and education-focused programs, the analysis in this chapter focused on sample members who did not have a high school diploma or GED when they entered the study.[1] This chapter asks to what extent additional months spent in adult education are positively associated with postprogram educational outcomes among this group of welfare recipients. Specifically, we address the following questions:

- What is the relationship between additional months of adult education and respondents' reading skills, math skills, and the likelihood of obtaining a GED by the end of a two-year follow-up period?
- How does this payoff[2] to additional months of adult education change depending on how many months of adult education an individual has already received? (That is, is the payoff to additional education constant over time? Does it taper off after some point? Or does it follow some other pattern?)

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[1] This sample includes individuals from the site in Riverside who possessed a GED or high school diploma but who scored below a certain threshold on the reading and math tests administered at the time of random assignment or had limited English, and thus were determined to be in need of basic education.

[2] The term *payoff* will be used throughout this chapter as a shorthand expression for the relationship between an additional month of adult education and such educational outcomes as reading and math achievement and GED receipt. While this term implies that there is a causal relationship between participation in adult basic education and the education outcomes, the authors acknowledge that other factors may explain the association. This issue will be discussed in more detail below.

- Does the relationship between participation in adult education and individuals' educational outcomes depend on whether it occurs on a voluntary basis or as part of a mandatory condition of their continued receipt of welfare benefits?
- Does the relationship between participation in adult education and educational outcomes vary across different subgroups of welfare recipients (defined in terms of levels of initial educational achievement, attitudes toward additional education, and family and personal barriers to participation)?
- What is the relationship between characteristics of adult education providers and the payoff to welfare recipients who participate in adult education?

II. Summary of the Findings

- **Our analyses suggest a positive relationship between time spent in adult education and educational outcomes. However, this relationship was not linear and varied for different outcomes and subgroups.**

Analyses presented in this chapter reveal that welfare recipients' educational outcomes were positively associated with time spent in adult education. This was true for measures of postprogram literacy and math achievement as well as GED receipt. However, this relationship changed as individuals spent more time in adult education programs.

There was no relationship between additional months of adult education and reading achievement during the first year of participation, but after the first year additional months of adult education were associated with significant increases in reading scores.

The relationship between math scores and participation in adult education followed almost the opposite pattern: there was no significant relationship between additional months of adult education after the first six months of participation, but there was a relationship during the first six months of participation. The magnitude of the relationship between time spent in adult education and literacy and math achievement outcomes was comparable to or greater than the relationship between these outcomes and time spent in formal education before random assignment.

The relationship between GED receipt and time spent in adult education followed a pattern similar to that of math scores. Early on, additional months of adult education increased GED receipt, but after six months of participation additional months were associated with lower levels of GED receipt.

- **The relationship between test scores and months in adult education was similar for program group members and for control group members.**

Both program and control group members experienced similar relationships between participation in adult education and educational outcomes. Thus, the payoff from adult education in terms of improved literacy and math skills was not related to whether participation was voluntary or a mandatory condition for the receipt or welfare benefits.

- **The relationship between months in adult education and GED receipt was stronger for program group members.**

Those mandated to participate in adult education experienced a stronger relationship between participation and GED receipt than those who volunteered.

- **The relationship between GED receipt and time spent in adult education was significantly stronger for those experiencing a more intensive program.**

Those who spent more than 20 hours a week in adult education experienced a stronger relationship between participation and GED receipt. This also appeared to be true of the link between participation and test scores, but that difference was not statistically significant.

- **The relationship between GED receipt and time spent in adult education was mostly limited to those entering adult education at the ninth grade level or higher.**

Among sample members who had completed eight or fewer years of formal education when they entered the study, there was almost no relationship between months of participation in adult education and the receipt of a GED or high school diploma. On the other hand, among individuals with more than eight years of formal education, there was a significantly larger and positive association between GED receipt and months in adult education.

- **Sample members with lower skill levels were not likely to receive a GED. However, longer participation in adult education increased the likelihood that they would.**

Individuals with lower levels of academic achievement (as measured by test scores) had a lower probability of receiving their GED than individuals with higher skills. However, after six months of participation they experienced a stronger relationship between GED receipt and total months spent in adult education.

- **Barriers to participation and motivation to participate in adult education did not affect the relationship between participation and subsequent educational outcomes.**

The relationship between educational outcomes and months of participation was unrelated to the presence of family and personal problems, to whether or not sample members had young children, or to whether or not they preferred attending school as opposed to some other activity. Moreover, the payoff to adult education in terms of improved literacy skills also did not vary significantly with individuals' initial academic preparation.

- **Teachers' experience and education levels appeared to enhance the benefits of participation in adult education as far as literacy and math scores were concerned. It did not similarly affect GED receipt.**

The relationship between months of participation and test score outcomes was stronger for sample members who attended classes at adult education providers where teachers were more experienced and were more likely to possess advanced degrees. However, the relationship between GED receipt and total months spent in adult education was not related to the number of staff with advanced degrees and was actually somewhat negatively associated with teachers' experience.

The remainder of the chapter proceeds as follows. Section III briefly reviews the data sources for this chapter and describes the welfare recipients who participated in adult education. Section IV outlines the analytic approach to answering the questions explored in this chapter. Section V presents the analysis of the relationship between educational outcomes and time spent in adult education, and section VI offers conclusions.

III. Samples and Data Sources

A. Welfare Recipient Data

The sample for this analysis is essentially the same as the sample used in Chapter 2, which includes data from the three programs in Atlanta, Grand Rapids, and Riverside. Within these programs, the analysis in this chapter is based on welfare recipients who were either part of a mandatory education-focused program or those who were randomly assigned to a control group. Control group members were excluded from the mandatory welfare-to-work program but could seek out services in the community on their own. Within this three-site sample, we focused on those without a high school diploma or GED, including in Riverside a group who did have a high school diploma or GED but who scored below a certain level on the reading and math tests administered at the time of random assignment.

A variety of data was collected covering these welfare recipients' demographic characteristics, attitudes, prior educational achievement and attainment, and personal circumstances. These data include reading and math achievement tests administered both at the outset of the study and two years later. For added detail on data sources, please refer to Chapter 2 and Appendix A.

B. Education Provider Data

As mentioned previously in this report, the NEWWS Evaluation extended data collection to adult education providers serving sample members in the three sites studied. In particular, field research was conducted—including interviews with administrators and teachers—in order to gather information on such provider characteristics as teachers' experience and training, curriculum and teaching methods, and program organization, among others. These characteristics were discussed in Chapter 2 and will be used here to explore whether provider characteristics influence the payoff to participation in adult education.

The agencies covered by the interviews and surveys were the largest providers of adult education in the three sites' welfare-to-work programs. We completed interviews with administrators at 33 agencies that served between 20 and 34 percent of sample members across the three sites. Although these providers interacted with only a minority of sample members, they served the majority of individuals who participated in adult education (from 74 percent in Atlanta to 89 percent in Riverside). Nevertheless, the fact that we did not have provider data for all sample members who participated in adult education did constrain the analysis and limited our ability to estimate relationships between provider characteristics and educational outcomes.

The primary focus of this chapter is an analysis of the relationship between the *extent* of participation in adult education and postprogram educational outcomes. Therefore, while we compared educational outcomes of those who reported any participation in adult education with those who did not, the *primary* sample for this chapter is the group of clients who actually participated in adult education activities. The analysis of the correlation between provider characteristics and the relationships between postprogram educational outcomes and total months spent in education will be based on a smaller sample of individuals who not only participated in adult education but also attended classes at education providers for which we have data on teacher and school characteristics.

Table 3.1 compares baseline characteristics of the sample as a whole with those of sample members who participated in adult education activities and attended classes at one of the providers for which we possess data. While the table indicates the existence of some differences across these groups, overall the groups appear to be fairly similar. In particular, respondents who participated in adult education activities were slightly more likely to have children age 5 or younger, were less likely to be black, and were more likely to be neither black nor white. They also were about a year and a half younger at the time of random assignment, had somewhat higher test scores, and were substantially more likely to have participated in some kind of education or training activity in the past 12 months.

IV. Analytic Approach

The basic approach to the analysis in this chapter is a type of "production function" approach that has been applied successfully in estimating the effects of education on academic achievement in a number of studies.[3] This approach hypothesizes that educational gains are a function of a variety of factors, including students' background characteristics; their prior academic achievement; and the nature, quality, and quantity of the educational resources to which they are exposed. This can be summarized as

$A_{it} = f(A_{i,t-1}, X_i, Att_i, S_i)$, where

A_{it} represents academic achievement at the time of the follow-up survey of student *i*, at adult education provider *j*, as measured by an achievement test score or some other measure;

A_{it-1} represents the baseline level of academic achievement or prior preparation of student *i*, again measured by an achievement test or some other measure;[4]

(continued)

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[3] See, for example, Ferguson, 1991; Ferguson and Ladd, 1996; Hanushek, 1972, 1986; and Murnane, 1981.

[4] Another approach to modeling students' achievement outcomes would be to use the students' achievement gains as the dependent variable, that is, $(A_{it} - A_{i,t-1}) = F(B_i, Att_i S_i)$. Provided that the coefficient associated with $A_{i,t-1}$ is equal to 1, this model is equivalent to the one illustrated in the text. However, we consider it preferable to allow the coefficient on baseline achievement to be estimated empirically rather than artificially restricting this coefficient to equal 1.

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Table 3.1

For Sample Members Without a High School Diploma or GED at Random Assignment:[a] Means of Background Characteristics for Respondents with Control Variables

Background Characteristic	Full Sample	Nonparticipants or No Provider Data	ABE Participants with Provider Data
Reading and math skills at random assignment			
TALS document literacy test score	264.35	262.53	270.70 ***
	(45.03)	(45.12)	(44.18)
CASAS math test score	208.94	208.41	210.79 ***
	(13.41)	(13.21)	(13.94)
Education status at random assignment			
Highest grade completed	10.33	10.38	10.17 ***
	(1.31)	(1.31)	(1.27)
Enrolled in education or training activity in 12 months prior to random assignment	0.18	0.15	0.26 ***
	(0.38)	(0.36)	(0.44)
Attitudes and opinions at random assignment[b]			
Family or Personal Problems scale	2.10	2.12	2.02 ***
	(0.65)	(0.66)	(0.61)
Parental Concerns scale	2.15	2.15	2.13
	(0.56)	(0.55)	(0.58)
Preference for School scale	2.25	2.28	2.14 ***
	(0.49)	(0.49)	(0.47)
Depressive Symptoms scale	2.13	2.16	2.06 **
	(0.93)	(0.93)	(0.91)
Mastery scale	2.22	2.22	2.21
	(0.58)	(0.57)	(0.58)
Family status at random assignment			
Ever married	0.46	0.45	0.49
	(0.50)	(0.50)	(0.50)
2 children	0.34	0.33	0.35
	(0.47)	(0.47)	(0.48)
3 or more children	0.31	0.31	0.32
	(0.46)	(0.46)	(0.47)
Any child 5 years old or younger	0.46	0.45	0.51 **
	(0.50)	(0.50)	(0.50)

(continued)

Table 3.1 (continued)

Background Characteristic	Full Sample	Nonparticipants or No Provider Data	ABE Participants with Provider Data
Public assistance and labor force status			
Cumulative years on public assistance	5.10	5.11	5.05
	(1.60)	(1.60)	(1.61)
First spell of AFDC receipt	0.13	0.12	0.15
	(0.33)	(0.32)	(0.35)
Any paid work in year prior to random assignment	1,072.44	1,068.45	1,086.38
	2,549.20	2,538.18	2,590.22
Demographic characteristics			
Female	0.97	0.96	0.98 *
	(0.18)	(0.19)	(0.14)
Age	31.06	31.41	29.84 ***
	(6.84)	(6.94)	(6.32)
Black	0.57	0.60	0.48 ***
	(0.50)	(0.49)	(0.50)
Not black or white	0.14	0.13	0.18 **
	(0.35)	(0.34)	(0.39)
Sample size	1,926	1,497	429

SOURCES: MDRC calculations from information routinely collected by welfare staff, TALS document literacy test and CASAS math test data, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally.

A two-tailed t-test was applied to the means listed above, comparing participants with provider data to those who were either nonparticipants or for whom provider data were not available. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

Numbers shown in parentheses are the standard deviations associated with the coefficient shown above.

[a] The calculations in this table include only sample members who had both baseline and follow-up test score data in the Atlanta, Grand Rapids, and Riverside education-focused programs and had participated in adult education within 2 years of random assignment for more than 1 month but less than 25 months. Individuals who participated for 25 months or more were excluded due to small sample sizes.

[b] These subgroups are based on scales created from Private Opinion Survey data. For an explanation of these subgroups see Appendix F.

X_i represents the background characteristics of student *i*, such as his or her initial level of motivation and the supports or barriers to academic achievement which exist in the environment;

S_i represents the characteristics of the training provider or educational institution which served student *i*; and

Att_i is a measure of the level of participation of student *i* in the education activities—in this case, months of attendance in adult education.

The analysis in this chapter focuses on variation in postprogram educational outcomes across individuals with different lengths of stay in adult education, controlling for factors such as motivation and other background characteristics. Among other things, this analysis will allow us to estimate the relationship between students' improvements in measured cognitive skills and the duration of their participation in adult education activities, controlling for prior achievement and the background characteristics discussed above. The basic model can be written as

$$A_{it} = \beta_0 + \beta_1 Att_i + \beta_2 A_{it-1} + \beta_3 X_i + r_{ij}.^{5} \qquad (1)$$

At this level of analysis, we are primarily interested in the relationship between postprogram achievement and total months of participation in the adult education activities during the two-year follow-up period. This is represented by β_1, the estimated payoff to an additional month in adult education.

This estimate draws on variation across individuals who spent different amounts of time in adult education activities. It is important to note that while our analysis controls for observed characteristics such as initial academic achievement and attitudes toward school, it is possible that individuals who spent more months in adult education were different from those who spent fewer months, in ways that were not directly observed and could not be controlled for in the analysis. For example, individuals who spent more time in adult education might have been more motivated to improve their academic skills than those who spent less time in adult education. To the extent that such differences existed and also directly affected postprogram outcomes, the analysis may not have accurately captured the causal relationship between educational outcomes and time spent in adult education. On the other hand, it is not clear how unobserved differences in factors such as motivation would have translated into different postprogram achievement levels except through additional participation in adult education activities.

One of the questions we intended to explore is whether or not the relationship between time spent in adult education and postprogram educational outcomes was affected by individual

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[5] In this equation, the notation of error term r_{ij} represents the assumption that the error in this equation consists of an individual error term and a provider-level error term. In other words, the error is specific to individual *i* at adult education provider *j*. In order to account for this, statistical estimation procedures included a series of dichotomous variables representing each adult education provider. (These variables are not shown in the equation presented above.) The resulting estimates, known also as "fixed effects" estimates, are net of any direct effects of unobserved provider characteristics on postprogram educational attainment and achievement from the error term in the equation; they therefore yield correctly estimated coefficients and standard errors.

background characteristics such as prior educational achievement, family and personal problems, or the presence of young children. In other words, does the payoff to additional months of adult education differ across subgroups of welfare recipients with different background characteristics? We addressed this question by adding "interactions" between individual characteristics and months spent in adult education to the earlier model, as shown:

$$A_{ij,t} = \beta_0 + \beta_1 Att_{ij} + \beta_2 A_{ij,t-1} + \beta_3 X_{ij} + \beta_4 (X_{ij} Att_{ij}) + r_{ij}, \qquad (2)$$

so that we could estimate the average payoff to additional months of adult education (represented by β_1) at the same time we estimated the *differences* in the payoff to an additional month of adult education across individuals with different background characteristics (represented by β_4).

The analysis in this chapter also seeks to ascertain how the relationship between educational outcomes and time spent in adult education varies with the characteristics of the adult education providers.[6] For example, is the payoff to months in adult education significantly higher at adult education providers with smaller classes? In order to answer this question we estimated models such as the following:

$$A_{it} = \beta_0 + \beta_1 Att_i + \beta_2 A_{i,t-1} + \beta_3 X_i + \beta_4 (X_i Att_i) + \beta_5 (S_i Att_i) + r_{ij}. \qquad (3)$$

In this equation S_i represents a provider characteristic—for example, the class size—for a provider at which individual *i* attended classes. Therefore β_5 represents the difference in the payoff to adult education across providers with different characteristics.[7] In other words, this coefficient estimates the relationship between provider characteristics such as class size and the payoff to an additional month spent in adult education.

V. Relationship Between Educational Attainment and Achievement and Participation in Basic Education

A. Exploratory Analysis

1. Math Achievement. As pointed out above, our primary analytical focus was to estimate the payoff to time spent in adult education, that is, the relationship between individuals' postprogram educational outcomes and the amount of time they spent in adult education activities during the two-year follow-up period. As a first look at this relationship, Figures 3.1, 3.2, and 3.3 plot the average postprogram CASAS math score, TALS reading score, and level of GED receipt (respectively) against two-month increments of participation in adult education.[8] The first point on Figure 3.1 reports average postprogram math achievement among individuals who reported no participation in adult education; the second point represents the average level of achievement among individuals with 1 or 2 months of participation; the third point on the graph represents average postprogram math achievement among individuals with 3 or 4 months of participation; and so on.[9]

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[6] While this basic formulation is quite common, it does not necessarily take into account or take advantage of the multilevel nature of the data. In particular, a hierarchical model—of the kind described by Raudenbush and Bryk, 1992—may be the most appropriate way to approach the estimation of the education production function described above. Unfortunately, we had insufficient data to carry out such an approach and, instead, relied on interaction terms between provider- and site-level variables to identify how these relationships varied with the characteristics of the adult education providers.

[7] As mentioned above, these estimates included specific intercepts (fixed effects) for *each provider*. Therefore, it was not possible to estimate differences in postprogram outcomes that resulted from *direct* effects of provider characteristics. Only the interaction between provider characteristics and the payoff to months of adult education was estimable.

The shape of Figure 3.1 suggests that the relationship between adult education and postprogram math achievement is flat for the first few months and is somewhat steeper between 4 and 8 months. In particular, the average math score among welfare recipients in the sample who reported no participation in adult education was about 208 points—about the same as those who reported either 1 to 2, or 3 to 4 months of participation. Among individuals who reported 7 to 8 months of participation, the score climbed to about 215 points. The CASAS has a standard deviation of approximately 14 points. Therefore, the 7-point difference between the test scores of respondents with 3 to 4 months of adult education and those with 7 to 8 months of adult education—translated into the "effect size" metric—can be thought of as a difference of approximately .50 of a standard deviation.

Figure 3.1 also shows that the payoff to months of adult education in terms of postprogram math scores begins to trend downward after this point, and it rises again a few months later. On the whole, however, the relationship after this point appears to be basically flat. Therefore Figure 3.1 suggests that while there is a slight delay in the payoff to additional months of adult education, the effect of adult education on postprogram math achievement is concentrated during the early months of participation and declines strongly in the second half of the first year of program attendance.

2. Reading Achievement. Figure 3.2 suggests a somewhat different pattern in the relationship between time spent in adult education and postprogram reading skills. During the first year of attendance, the relationship between months spent in adult education and subsequent reading scores is weak or insubstantial. However, after one year the relationship appears to turn upward. Specifically, the average postprogram TALS document literacy test score among respondents with no participation in adult education was about 249 points. Among those with 11 to 12 months of participation in adult education, this score was essentially unchanged (247 points). However, for those who reported 17 to 18 months of participation in education, the average TALS reading score was 274 points—approximately 27 points higher. Inasmuch as the standard deviation of the TALS among respondents in our sample was approximately 49, this difference translates into an effect size of .55 of a standard deviation.

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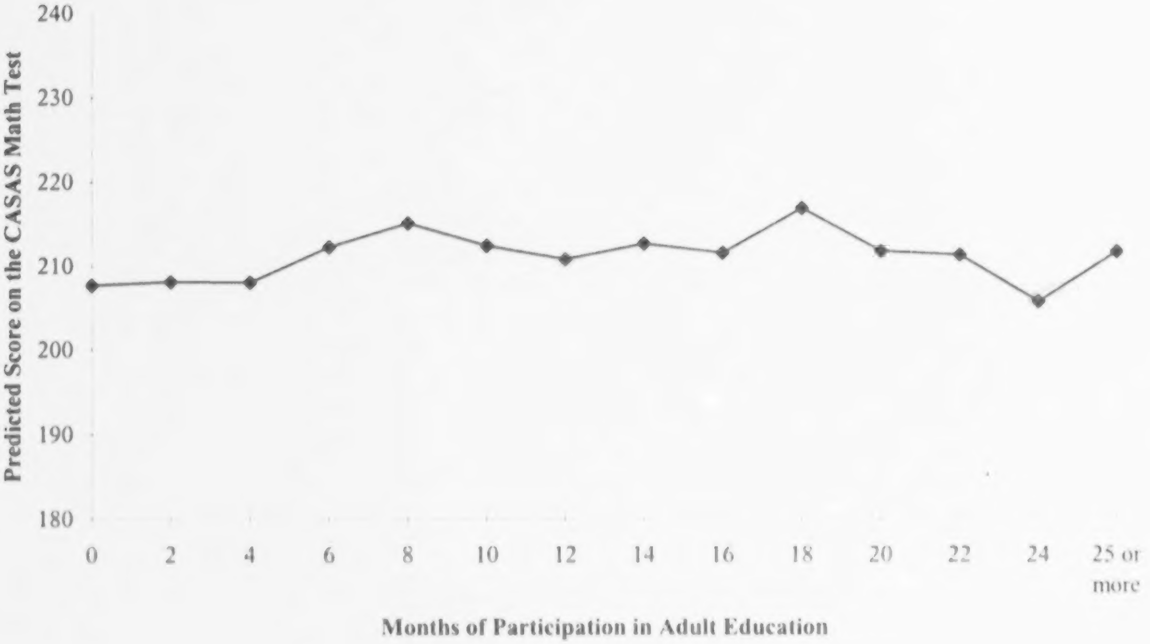
[8] Specifically, follow-up TALS scores, CASAS scores, and GED receipt were regressed on a series of dichotomous variables representing eight-week increments of participation in adult education and a series of individual-level control variables for background characteristics. These regression estimates were combined with sample averages in order to generate predicted values for sample members with average characteristics.

[9] The graphs of reading and math achievement are scaled so the extreme values on the vertical axes each represent points two standard deviations above and below the mean, respectively. Thus, rather than representing the total universe of possible test scores, they represent 95 percent of the distribution of test scores that was actually observed in our sample.

Figure 3.1

For Sample Members Without a High School Diploma or GED at Random Assignment:[a] Predicted Score on CASAS Math Test Two Years After Random Assignment, by Months of Participation in Adult Education



SOURCES: MDRC calculations from information routinely collected by welfare staff, TALS document literacy test and CASAS math test data, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Adult education includes ABE, GED, ESL, and high school completion classes.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally.

The y-axis spans approximately plus or minus 2 standard deviations from the mean TALS document literacy score.

[a]The calculations in this table include only sample members who had both baseline and follow-up test score data in the Atlanta, Grand Rapids, and Riverside education-focused programs.

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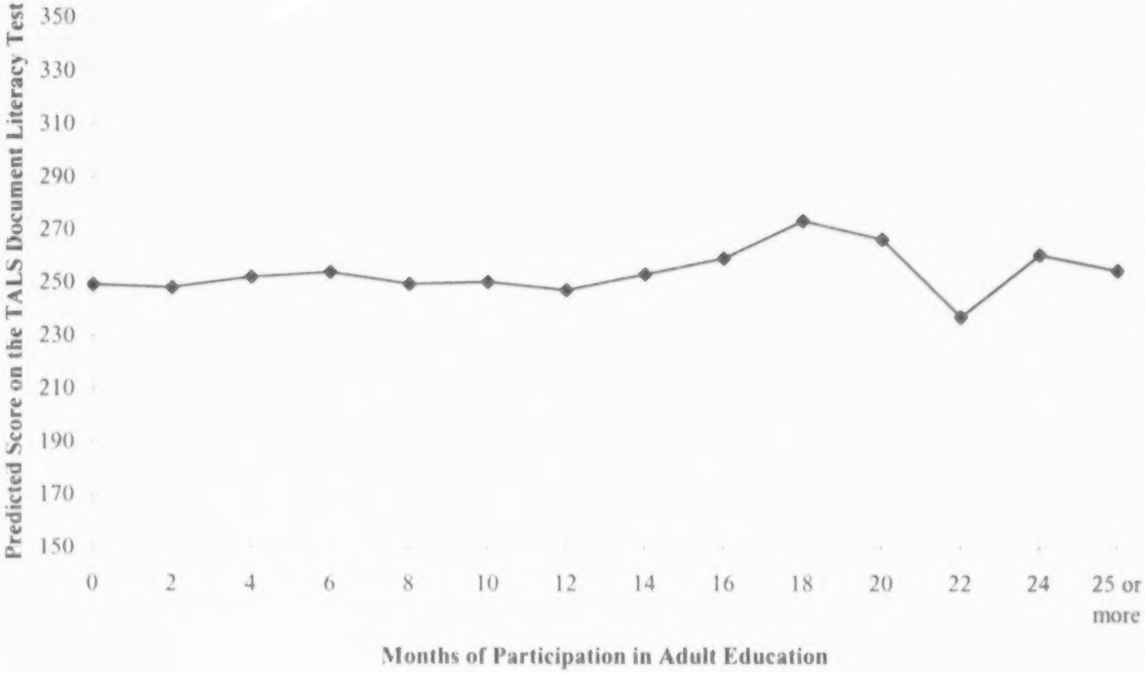
Figure 3.2

For Sample Members Without a High School Diploma or GED at Random Assignment:[a] Predicted Score on TALS Document Literacy Test Two Years After Random Assignment, by Months of Participation in Adult Education



SOURCES: MDRC calculations from information routinely collected by welfare staff, TALS document literacy test and CASAS math test data, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Adult education includes ABE, GED, ESL, and high school completion classes.

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To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally.

The y-axis spans approximately plus or minus 2 standard deviations from the mean TALS document literacy score.

[a] The calculations in this table include only sample members who had both baseline and follow-up test score data in the Atlanta, Grand Rapids, and Riverside education-focused programs.

This suggests several possibilities. Among the first that comes to mind is that those who remained in adult education for more than 12 months were somehow different from the participants who left adult education before that point. One might argue that, even controlling for observable background characteristics, students who remained in adult education were likely to be more motivated and better prepared and thus were more likely to score highly on postprogram achievement tests, regardless of their participation in adult education and regardless of their preprogram test. However, if the differences in test scores were a function of this sort of "selection bias," we might expect that each additional month of adult education would be associated with better preparation and higher levels of motivation. The fact that there was *no observed relationship* between months in adult education and educational achievement for the first 12 months of participation suggests that this was not the case. Moreover, if those who stayed in adult education beyond a year had different underlying unobserved characteristics than their counterparts who did not, we might have expected a shift in the line, rather than the substantial change in the slope of the line shown in Figure 3.2.[10]

Finally, to the extent that those who remained in adult education between a year and 18 months were somehow "different" or more motivated than other students, we would expect to see higher levels of both reading *and* math achievement for these individuals, not just higher literacy levels. As Figure 3.1 revealed, math scores did not increase after the first 12 months. Therefore, it is difficult to argue that unmeasured underlying characteristics accounted for the apparent effects of long-term participation in literacy scores. If this were the case, these characteristics would somehow have had to affect measured literacy without affecting math scores as well.

Therefore, rather than merely reflecting the selection of a group of individuals who were inherently different from their counterparts with fewer months of participation, the pattern in Figure 3.2 suggests that effects of participation in adult education on literacy may have been weak at first but may have increased substantially after a year of participation in adult education. This suggests that a threshold level of services was needed to substantially improve the literacy skills of this sample. It may take a great deal of consistent effort at establishing a basic foundation before participation in a literacy program has any substantial benefit or a meaningful effect on skills measured by the TALS. On the other hand, math achievement may improve at a relatively early stage of participation in adult education, but it does not appear to improve after a year in the program. This suggests that while basic math skills may have been relatively easy to improve, students' either reached a plateau in their progression toward better math skills or the programs shifted their focus away from math skills and toward literacy skills as students mastered basic math and spent more time in the program.

Finally, Figure 3.2 indicates a downward trend in reading test scores after 18 months in adult education. This pattern does not differ substantially from the pattern observed in Figure 3.1 regarding math scores, as well as the pattern which will be illustrated below regarding GED receipt. Unless we believe that, all other things being held constant, adult education could actually depress educational achievement, it is reasonable to conclude that the apparent downward trend near the end of the observation period represents a kind of negative "selection bias." In particular, individuals with long spells of participation may have been unable to progress in their adult education classes, because of lack of motivation, learning disorders, or other barriers.

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[10] It is also possible that the length of time between follow-up testing and school exit may have had a negative effect on test scores. If, for example, the test scores of students who exited one program quickly were biased downward by taking the test several months later, the estimated payoff to adult education among those individuals might also be biased downward.

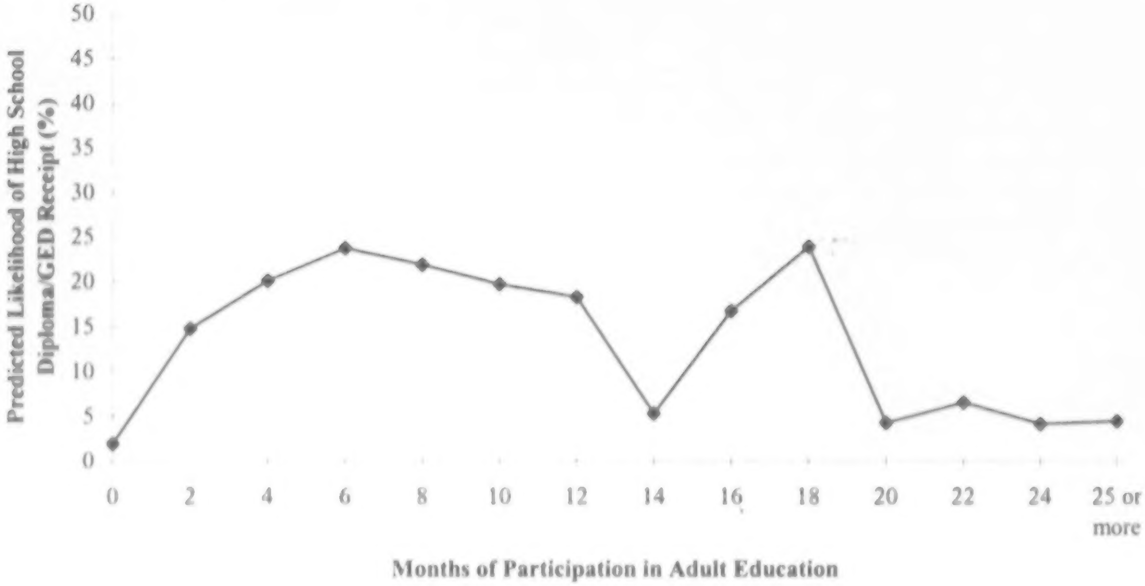
These findings may help to explain why the estimates from Chapter 2 suggest that assignment to an education-focused program increased participation in adult education without increasing postprogram test scores. In particular, while this analysis suggests a relationship between time spent in adult education and postprogram reading and math scores, it also suggests that this effect was delayed and subject to reaching a given threshold level of participation that relatively few sample members reached. In particular, there does not appear to have been a substantial relationship between postprogram TALS literacy scores and additional months spent in adult education during the first 12 months. While the payoff to adult education in terms of math scores appears to occur sooner than for reading, there still was a period of several months during which, according to Figure 3.1, the relationship between postprogram math scores and months of adult education also appeared to be relatively flat. The impacts on participation reported in Chapter 2 amounted to about two and a half months of participation in adult education, and most of this seemed to occur during the first few months of respondents' participation in the program. Therefore, it is not surprising that these did not translate into statistically significant impacts on student achievement.

3. GED Receipt. Figure 3.3 illustrates what might be considered the "credential" effect of time spent in adult education. It shows the relationship between the number of months spent in adult education and receipt of a GED during the follow-up period. The relationship during the first year or so followed a somewhat "bell-shaped" pattern. In particular, there was a steep positive relationship at the outset, followed by a range in which the relationship between participation and GED receipt was relatively flat but declining. This, in turn, was followed by a range of more steeply declining returns nearer the end of the first year, which was followed by another, shorter bell-shaped pattern. Specifically, about 2 percent of those who did not participate in adult education received a GED, while approximately 15 percent of those who spent 1 or 2 months in adult education received a GED. It seems unlikely that this initial difference of 13 percentage points was entirely the result of the education received by participants. Instead, it would appear that underlying differences between those who participated in education and those who did not accounted for at least part of the effect. Also, it is plausible that a number of short-term participants in adult education entered the program, discovered that they were close to getting a GED, took the test, passed, and left. Those who never participated might not have had such an opportunity.

The differences between 1 to 2 months and 5 to 6 months of participation in adult education indicate that the relationship flattens out after the first month but is still positive. In particular, 24 percent of those who reported 5 to 6 total months of participation in adult education during the follow-up period reported receiving their GED or high school diploma during this time—an increase of 9 percentage points. The line slopes downward to 18 percent by about 12 months, and then it experiences a steep dip to 5 percent at 13 to 14 months. Next, the likelihood of obtaining a GED during the follow-up period experiences a relatively sharp increase between 13 to 14 months and 17 to 18 months, and it drops off substantially after that.

Figure 3.3

For Sample Members Without a High School Diploma or GED at Random Assignment:[a] Predicted Likelihood of High School Diploma or GED Receipt Within Two Years After Random Assignment, by Months of Participation in Adult Education



SOURCES: MDRC calculations from information routinely collected by welfare staff, TALS document literacy test and CASAS math test data, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Adult education includes ABE, GED, ESL, and high school completion classes.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally.

The y-axis spans approximately plus or minus 2 standard deviations from the mean TALS document literacy score.

[a] The calculations in this table include only sample members who had both baseline and follow-up test score data in the Atlanta, Grand Rapids, and Riverside education-focused programs.

The interpretation of this pattern is problematic, because the relationship between GED receipt and participation in adult education can be reciprocal: adult education prepared sample members for the GED, increasing their likelihood of passing the test; in most cases, however, GED receipt also would have ended participation in adult education, thereby reducing time spent in it. Especially for sample members who spent long periods in adult education, failure to pass the GED test may have caused their participation spells to be lengthened. This explanation for the negative relationship between long-term participation and reduced GED receipt is more plausible than an explanation which posits that more education would make one less likely to take or pass a GED test.

B. Relationship Between Postprogram Educational Outcomes and Time Spent in Adult Education

The previous section described the observed patterns in the relationship between reading scores, math scores, GED receipt, and time spent in adult education. The analysis presented below built on this evidence in two ways. First, we estimated the effect of additional months of adult education on educational outcomes. Second, we evaluated how this payoff to additional education varied with the amount of adult education already accumulated. In other words, was the payoff to an additional month of education different in the first month of participation than in the sixth or twelfth month? Finally, we explored the extent to which the estimated payoff to adult education varied across different groups of welfare recipients, depending on their background characteristics, personal circumstances, and other factors.

1. Reading Achievement. Table 3.2 reports regression estimates of the relationship between additional months of adult education and students' postprogram scores on the Test of Applied Literacy Skills (TALS) document literacy test, controlling for baseline academic achievement and other background characteristics. The coefficients in this table represent the difference in postprogram TALS scores associated with a one-unit increase in each variable, all other variables in the table being equal.

The equation whose coefficients are presented in column 1 of Table 3.2 includes a variable indicating whether or not the respondent reported at least one month of participation in adult education during the follow-up period and, beyond that point, a variable equal to the number of additional months of adult education. The coefficient associated with the variable for ever participating represents the difference between those who spent any time in adult education during the two-year follow-up period and those who received no adult education at all. The coefficient associated with additional months of participation represents the average difference per month in postprogram test scores across adult education *participants* who reported different amounts of participation during the follow-up period. This might be interpreted as the average payoff to a month of adult education.

The estimates shown in column 1 indicate the existence of a significant relationship between postprogram literacy skills and total months in adult education. In particular, the estimates indicate that, among individuals with at least one month of adult education, an additional month of adult education is associated with a difference of .57 points on the postprogram TALS reading

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Table 3.2

For Sample Members Without a High School Diploma or GED at Random Assignment:[a] Relationship Between Participation in Adult Education Activities and Two-Year TALS Document Literacy Scores

Independent Variable	Coefficient Model 1	Coefficient Model 2
Participation in adult education within 2 years of random assignment		
1 month of participation	3.35	3.91
	(6.11)	(6.31)
Total months, beyond 1	0.57 **	-
	(0.23)	-
Months beyond 1, through 6	-	1.10
	-	(0.85)
Months beyond 6, through 12	-	-0.96
	-	(1.00)
Months beyond 12, through 18	-	3.30 *
	-	(1.75)
More than 18 months		-5.25
	-	(9.89)
Reading and math skills at random assignment		
TALS document literacy test score	0.52 ***	0.52 ***
	(0.03)	(0.03)
CASAS math test score	0.47 ***	0.47 ***
	(0.08)	(0.08)
Education status at random assignment		
Highest grade completed	1.89 ***	1.89 ***
	(0.70)	(0.70)
Enrolled in education or training activity in 12 months prior to random assignment	1.56	1.70
	(2.12)	(2.12)
Attitudes and opinions at random assignment[b]		
Family or Personal Problems scale	0.62	0.56
	(1.74)	(1.74)
Parental Concerns scale	-2.00	-1.87
	(2.02)	(2.02)
Preference for School scale	0.38	0.48
	(1.81)	(1.81)
Depressive Symptoms scale	-0.03	-0.15
	(0.93)	(0.93)
Mastery scale	-3.91 **	-3.83 **
	(1.73)	(1.73)

(continued)

Table 3.2 (continued)

	Coefficient	
Independent Variable	Model 1	Model 2
Family status at random assignment		
Ever married	-0.37	-0.24
	(1.98)	(1.98)
2 children	-0.16	0.01
	(2.04)	(2.04)
3 or more children	2.20	2.15
	(2.24)	(2.24)
Any child 5 years old or younger	-1.66	-1.73
	(2.01)	(2.01)
Public assistance and labor force status at random assignment		
Cumulative years on public assistance	-0.98	-0.94
	(0.64)	(0.64)
First spell of AFDC receipt	2.60	2.54
	(2.64)	(2.64)
Any paid work in year prior to random assignment	0.00	0.00
	(0.00)	(0.00)
Demographic characteristics		
Female	5.00	5.03
	(5.02)	(5.02)
Age	-0.60 ***	-0.60 ***
	(0.16)	(0.16)
Black	-13.02 ***	-12.90 ***
	(2.41)	(2.41)
Not black or white	-1.89	-2.03
	(2.86)	(2.86)
R-squared	0.47	0.47
Sample size	1,955	1,955

(continued)

Table 3.2 (continued)

SOURCES: MDRC calculations from information routinely collected by welfare staff, TALS document literacy test and CASAS math test data, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Adult education includes ABE, GED, ESL, and high school completion classes.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

Survey respondents were interviewed between month 25 and month 31, counting the month in which random assignment occurred as month 1.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally.

A two-tailed t-test was applied to coefficients listed above. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

Numbers shown in parentheses are the standard errors associated with the coefficient shown above.

[a] The calculations in this table include only sample members who had both baseline and follow-up test score data in the Atlanta, Grand Rapids, and Riverside education-focused programs and had participated in adult education within 2 years of random assignment for more than 1 month but less than 21 months. Individuals who participated for 21 months or more were excluded due to small sample sizes.

[b] These subgroups are based on scales created from Private Opinion Survey data. For an explanation of these subgroups see Appendix E.

test. Moreover, this difference is statistically significant at the 5 percent level.[11] Taken by itself, the average payoff to an additional month of adult education might seem relatively small compared to the distribution of test scores in our sample. In particular, in terms of effect size, the estimated effect is only .012 of a standard deviation.

However, after six months, this effect would translate into a difference of 3.42 points, or .07 of a standard deviation. As a basis for comparison, we estimated the relationship between sample members' baseline reading and math test scores and the highest grade completed at the time of random assignment. These estimates suggest that, in this sample, an additional year of formal education was associated with a difference of 8.3 points, or .17 of a standard deviation, on the TALS. This suggests that the average literacy payoff to six months in adult education was slightly more than 40 percent of the payoff to an average academic year of the schooling the individuals in our sample received prior to entering the Job Opportunities and Basic Skills Training (JOBS) program.

As the graphs presented earlier suggested, the payoff to additional months of adult education appeared to vary according to sample members' length of stay in adult education. For example, the difference in literacy achievement between individuals with 13 as opposed to 12 months of adult education might be greater than the difference between individuals with 1 and 2 months of adult education. In order to account for this, we estimated a different version of the equation linking participation in adult education and postprogram test scores, allowing the payoff to time spent in adult education to vary across six-month intervals of participation.[12] The results are presented in column 2 of Table 3.2.

The estimates in column 2 reveal a pattern that is consistent with the one first seen in Figure 3.2. It appears that the association between postprogram reading scores and additional months spent in adult education during the first year was not statistically distinguishable from zero. However, the estimated effect of an additional month of participation between months 13 and 18 was both statistically significant and substantively important. In particular, each additional month after a year of participation was associated with a 3.3 point gain in the postprogram reading scores (for an effect size of .067). Based on this estimate, the payoff to six additional months of adult education after the first year would be approximately 19.8 points, or .40 of a standard deviation, on the TALS document literacy test.

It may be useful once again to compare these differences with the differences in baseline literacy scores associated with years of formal education before participation in the JOBS program. As mentioned above, a year of formal education before the beginning of the program was associated with a difference of 8.3 points, or .17 of a standard deviation, in baseline TALS scores. If we assume that each academic year of formal education consisted of approximately 9 months of schooling, this estimate suggests that 18 months of formal education was associated with 16.6 points, or .34 of a standard deviation, on the TALS. In other words, the difference in TALS literacy associated with 18 months of adult education appears to be similar to the difference in literacy scores associated with 18 months of formal preprogram education. In short, it appears that while there is no significant relationship between short-term participation in adult education and postprogram literacy achievement, there is a *substantial* association between literacy outcomes and longer-term participation in adult education. Table 3.5 further illustrates this point by presenting the predicted TALS scores (as well as other outcomes) for sample members with 0, 1, 6, 12, and 18 months of adult education.

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[11] In other words, the chances that the data would yield an estimate of this magnitude, given that the true relationship is zero, are less than 5 percent.

[12] The estimate in column 2 of the table also includes an indicator variable for whether or not an individual spent more than 18 months in adult education. Because few individuals participated for that long, it was not possible to reliably estimate the incremental effect of an additional month beyond this point. Therefore, we included a dichotomous variable to capture the average achievement level for this group.

2. Math Achievement. Table 3.3 presents regression estimates of the relationship between total months in adult education and postprogram Comprehensive Adult Student Assessment System (CASAS) math scores. These estimates were generated in the same manner as those presented in Table 3.2. However, as was the case when we compared Figures 3.1 and 3.2, the estimates in this table suggest that the relationship between months in adult education and postprogram math scores differed from that which was observed for postprogram literacy scores. Our analysis suggested a relationship between participation in adult education and postprogram literacy that was weak during the early months and became stronger after the first year. The relationship between participation in adult education and postprogram math scores, on the other hand, suggests a relationship that was somewhat stronger during the early months of participation and weakened over time.

The estimated relationship between total months of participation during the two-year follow-up and postprogram math scores, presented in column 1 of Table 3.3, suggests that, on average, an additional month of adult education completed during the two years following random assignment was associated with a difference of .28 points, or .020 of a standard deviation, in postprogram CASAS math scores. Thus, six months of adult education were associated with a gain of 1.68 points, or .12 of a standard deviation, on the CASAS. As mentioned earlier, we estimated the relationship between sample members' accumulated education at program entry and their baseline test scores. According to these estimates, an additional year of education was associated with a difference of 1.43 points, or .10 of a standard deviation, on the CASAS. Therefore, on average, the payoff to six months of adult education in terms of math skills was as large as or larger than the payoff to an average year of the formal schooling received before entering the study.

As in Table 3.2, the second column of Table 3.3 explores the possibility that the relationship between months of education and postprogram achievement was nonlinear. The estimates again reflect the simple pattern presented in graphic form in an earlier section. As in Figure 3.1, the estimates indicate the existence of a significant positive relationship between additional education and math achievement in the first six months, and no such relationship thereafter. In particular, after the first month spent in adult education, each additional month through the sixth was associated with a difference of .78 points, or .056 of a standard deviation, on the CASAS. According to this estimate, the difference in postprogram math achievement between someone who participated in adult education for one month and someone who participated for six months was 3.9 points, or .28 of a standard deviation.

After the first year of participation, there were no statistically significant gains in math scores associated with additional months of participation in adult education. In other words, our estimates predicted significant gains in math skills for those completing 6 months of adult educa-

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Table 3.3

For Sample Members Without a High School Diploma or GED at Random Assignment:[a] Relationship Between Participation in Adult Education Activities and Two-Year CASAS Math Scores

	Coefficient	
Independent Variable	Model 1	Model 2
Participation in adult education within 2 years of random assignment		
1 month of participation	-0.83	0.42
	(2.64)	(2.69)
Total months, beyond 1	0.28 ***	-
	(0.10)	-
Months beyond 1, through 6	-	0.78 **
	-	(0.34)
Months beyond 6, through 12	-	0.35
	-	(0.43)
Months beyond 12, through 18	-	0.49
	-	(0.74)
More than 18 months	-	-6.12
	-	(4.23)
Reading and math skills at random assignment		
TALS document literacy test score	0.10 ***	0.10 ***
	(0.01)	(0.01)
CASAS math test score	0.28 ***	0.28 ***
	(0.03)	(0.03)
Education status at random assignment		
Highest grade completed	0.54 *	0.51 *
	(0.28)	(0.28)
Enrolled in education or training activity in 12 months prior to random assignment	0.02	-0.10
	(0.88)	(0.88)
Attitudes and opinions at random assignment[b]		
Family or Personal Problems scale	0.66	0.62
	(0.72)	(0.72)
Parental Concerns scale	0.81	0.79
	(0.89)	(0.89)
Preference for School scale	-2.25 ***	-2.22 ***
	(0.72)	(0.72)
Depressive Symptoms scale	0.39	0.41
	(0.38)	(0.38)
Mastery scale	-1.24 *	-1.26 *
	(0.70)	(0.70)

(continued)

Table 3.3 (continued)

	Coefficient	
Independent Variable	Model 1	Model 2
Family status at random assignment		
Ever married	0.23	0.27
	(0.84)	(0.84)
2 children	0.52	0.46
	(0.82)	(0.82)
3 or more children	0.97	0.79
	(0.92)	(0.92)
Any child 5 years old or younger	0.00	-0.14
	(1.12)	(1.13)
Public assistance and labor force status at random assignment		
Cumulative years on public assistance	-0.39	-0.34
	(0.25)	(0.25)
First spell of AFDC receipt	0.58	0.87
	(1.18)	(1.18)
Any paid work in year prior to random assignment	0.00	0.00
	(0.00)	(0.00)
Demographic characteristics		
Female	-1.07	-1.14
	(1.61)	(1.60)
Age	-0.13 **	-0.13 **
	(0.06)	(0.06)
Black	-3.51 ***	-3.47 ***
	(1.00)	(1.00)
Not black or white	-1.79	-1.86
	(1.22)	(1.22)
R-squared	0.45	0.45
Sample size	1,139	1,139

(continued)

Table 3.3 (continued)

SOURCES: MDRC calculations from information routinely collected by welfare staff, TALS document literacy test and CASAS math test data, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Adult education includes ABE, GED, ESL, and high school completion classes.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

Survey respondents were interviewed between month 25 and month 31, counting the month in which random assignment occurred as month 1.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally.

A two-tailed t-test was applied to coefficients listed above. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

Numbers shown in parentheses are the standard errors associated with the coefficient shown above.

[a] The calculations in this table include only sample members who had both baseline and follow-up test score data in the Atlanta, Grand Rapids, and Riverside education-focused programs and had participated in adult education within 2 years of random assignment for more than 1 month but less than 21 months. Individuals who participated for 21 months or more were excluded due to small sample sizes.

[b] These subgroups are based on scales created from Private Opinion Survey data. For an explanation of these subgroups see Appendix E.

tion, as opposed to none, but no significant differences in math scores for those completing 18 months, as opposed to 12. Table 3.5 further illustrates this point by presenting the predicted CASAS scores for sample members with 0, 1, 6, 12, and 18 months of adult education.

3. GED Receipt. Table 3.4 presents estimates of the relationship between participation in adult education and GED receipt during the follow up-period. Again, the differences between the estimates in columns 1 and 2 of this table highlight the nonlinear nature of this relationship. The first column reveals that, on average, an additional month of adult education was associated with an increase of .49 percentage points in the likelihood of receiving a GED or a high school diploma during the follow-up period. This suggests that a difference of six months in the time spent in adult education was associated with an average difference of 2.94 percentage points in the likelihood of obtaining a GED during the follow-up period.

Column 2 reveals that this modest association between months spent in adult education and subsequent GED receipt represented a strong positive association during the first six months of participation, followed by a somewhat negative association after that. In particular, from the first through the sixth month, each additional month of adult education was associated with an increase of 4.18 percentage points in the average likelihood of obtaining a GED during the follow-up period. This means that those who participated for six months had a likelihood of receiving a GED that was 21 percentage points higher than those who participated for only one month. On the other hand, each additional month of adult education from the seventh through the twelfth month of participation was associated with a reduction of 1.28 percentage points in this likelihood of receiving a GED during the follow-up period. A similar negative relationship was found for participation spells between 13 and 18 months in duration, but this relationships was not statistically significant.

Table 3.5 further illustrates this point (and summarizes earlier findings regarding reading and math scores) by presenting the estimated percentage of individuals who received their GED for sample members with 0, 1, 6, 12, and 18 months of adult education, respectively. Overall, this pattern suggests that there was a substantial positive association between months spent in adult education and receipt of a GED or high school diploma during the first six months of adult education. After that, this relationship weakened, becoming substantially negative over time. This suggests the possibility that individuals who stayed in adult education for longer periods may have been more difficult to serve, because of the presence of learning disabilities, their attitudes toward education, or other factors that made it more difficult for them to move toward the completion of a GED. On the other hand, individuals who received their GED after only a few months of adult education may have needed only a brief "refresher" course in order to pass the exam. As noted earlier, the interpretation of this apparent relationship is complicated by the fact that GED receipt may have *caused* spells of adult education to be shortened. This would dampen any positive association between participation in adult education and receipt of the credential and could explain why estimates describing this association became negative in the long run.

C. The Effectiveness of Mandatory Adult Education

Among other things, the NEWWS Evaluation was designed to evaluate the effectiveness of various *mandatory* welfare-to-work programs, including those that emphasize the receipt of additional education as a first step toward self-sufficiency. One of the primary questions with re-

Table 3.4

For Sample Members Without a High School Diploma or GED at Random Assignment:[a] Relationship Between Participation in Adult Education Activities and GED Receipt at Two-Year Follow Up

	Coefficient	
Independent Variable	Model 1	Model 2
Participation in adult education within 2 years of random assignment		
1 month of participation	-0.64	6.43
	(4.04)	(4.11)
Total months, beyond 1	0.49 ***	-
	(0.15)	-
Months beyond 1, through 6	-	4.18 ***
	-	(0.55)
Months beyond 6, through 12	-	-1.28 *
	-	(0.65)
Months beyond 12, through 18	-	-1.45
	-	(1.14)
More than 18 months	-	6.10
	-	(6.44)
Reading and math skills at random assignment		
TALS document literacy test score	0.05 ***	0.05 ***
	(0.02)	(0.02)
CASAS math test score	0.28 ***	0.28 ***
	(0.05)	(0.05)
Education status at random assignment		
Highest grade completed	0.16	0.17
	(0.46)	(0.45)
Enrolled in education or training activity in 12 months prior to random assignment	2.83 **	2.63 *
	(1.40)	(1.38)
Attitudes and opinions at random assignment[b]		
Family or Personal Problems scale	-1.82	-1.50
	(1.14)	(1.13)
Parental Concerns scale	2.10	1.76
	(1.32)	(1.31)
Preference for School scale	0.50	0.62
	(1.20)	(1.18)
Depressive Symptoms scale	1.85 ***	1.86 ***
	(0.61)	(0.61)
Mastery scale	-2.00 *	-2.21 **
	(1.14)	(1.13)

(continued)

Table 3.4 (continued)

Independent Variable	Coefficient	
	Model 1	Model 2
Family status at random assignment		
Ever married	-1.81	-1.63
	(1.31)	(1.29)
2 children	-0.90	-0.64
	(1.34)	(1.33)
3 or more children	-0.49	-0.49
	(1.48)	(1.46)
Any child 5 years old or younger	-0.14	-0.16
	(1.33)	(1.31)
Public assistance and labor force status at random assignment		
Cumulative years on public assistance	-0.49	-0.36
	(0.42)	(0.42)
First spell of AFDC receipt	5.45 ***	5.87 ***
	(1.74)	(1.72)
Any paid work in year prior to random assignment	0.00	0.00
	(0.00)	(0.00)
Demographic characteristics		
Female	2.39	2.50
	(3.30)	(3.26)
Age	-0.14	-0.10
	(0.10)	(0.10)
Black	-1.28	-1.41
	(1.59)	(1.57)
Not black or white	-1.57	-1.38
	(1.89)	(1.86)
R-squared	0.24	0.30
Sample size	1,962	1,962

(continued)

Table 3.4 (continued)

SOURCES: MDRC calculations from information routinely collected by welfare staff, TALS document literacy test and CASAS math test data, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Adult education includes ABE, GED, ESL, and high school completion classes.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

Survey respondents were interviewed between month 25 and month 31, counting the month in which random assignment occurred as month 1.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally.

A two-tailed t-test was applied to coefficients listed above. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

Numbers shown in parentheses are the standard errors associated with the coefficient shown above.

[a] The calculations in this table include only sample members who had both baseline and follow-up test score data in the Atlanta, Grand Rapids, and Riverside education-focused programs and had participated in adult education within 2 years of random assignment for more than 1 month but less than 21 months. Individuals who participated for 21 months or more were excluded due to small sample sizes.

[b] These subgroups are based on scales created from Private Opinion Survey data. For an explanation of these subgroups see Appendix F.

Table 3.5

For Sample Members Without a High School Diploma or GED at Random Assignment:[a] Predicted Postprogram Test Scores and GED Receipt, by Months of Participation in Adult Education

	Months of Participation				
Outcome	0 Months	1 Month	6 Months	12 Months	18 Months
GED receipt	1.64	8.07	28.97	21.29	12.59
Difference		6.43	20.90	-7.67	-8.70
Effect size[b]		0.27	0.87	-0.32	-0.36
Sample size		1,962			
TALS document literacy test score	247.23	251.15	256.65	250.91	270.71
Difference		3.91	5.50	-5.74	19.80
Effect size[b]		0.08	0.11	-0.12	0.40
Sample size		1,955			
CASAS math test score	206.76	207.18	211.10	213.17	216.11
Difference		0.42	3.92	2.07	2.94
Effect size[b]		0.03	0.28	0.15	0.21
Sample size		1,139			

SOURCES: MDRC calculations from information routinely collected by welfare staff, TALS document literacy test and CASAS math test data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Adult education includes ABE, GED, ESL, and high school completion classes.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

Survey respondents were interviewed between month 25 and month 31, counting the month in which random assignment occurred as month 1.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally.

A two-tailed t-test was applied to coefficients listed above. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

[a] The calculations in this table include only sample members who had both baseline and follow-up test score data in the Atlanta, Grand Rapids, and Riverside education-focused programs and had participated in adult education within 2 years of random assignment for more than 1 month but less than 21 months. Individuals who participated for 21 months or more were excluded due to small sample sizes.

[b] The effect size equals the difference divided by the standard deviation of the outcome measure for the control group. The standard deviations for control group members are: 24 for GED receipt; 49 for the TALS document literacy test score; and 14 for the CASAS math test score.

spect to these programs is whether the benefits of mandatory participation in adult education are the same as the benefits of voluntary participation. If, for example, welfare recipients who participated in adult education as part of a mandatory program were significantly less likely to benefit from this participation than those whose participation was entirely voluntary, the implication would be that welfare agencies are limited in their ability to better prepare individuals for the labor market by requiring them to improve their education.

As was mentioned earlier in this chapter, at the outset of the evaluation, individuals in the three-site sample were randomly assigned either to a mandatory education-focused welfare-to-work program or to a control group whose members could access available education and training services on their own. Even though they were not required to do so, some members of the control group reported participating in adult education activities during the two-year follow-up period. The following analysis compares the payoff to participation in adult education for members of both the program and the control groups to ascertain whether and how this payoff varied depending on whether or not participation was mandatory.

Specifically, the analyses of the relationship between additional months of adult education and the educational outcomes discussed above were repeated, allowing the payoff to adult education activities to vary according to whether or not individuals were part of the program group or the control group. Based on these regression estimates, Tables 3.6 and 3.7 present predicted levels of reading scores and GED receipt for individuals who spent different amounts of time in adult education, by research group. Note that because the postprogram CASAS test scores were administered only to a subgroup of the sample, the sample size was not large enough to allow us to conduct subgroup analyses for this outcome.[13]

1. Reading Skills. The first panel of Table 3.6 shows predicted postprogram TALS scores among the individuals in mandatory education-focused programs and among individuals who were in the control group, at 1, 6, 12, and 18 months of participation in adult education. The table also reports the results of statistical tests evaluating the hypothesis that the relationships shown were the same across these two groups. It appears that the payoff to adult education was similar for both groups, with effects somewhat stronger for control group members in the early months and for program group members in the later months. While the difference was not statistically significant, this pattern suggests that the program mandate may have improved the payoff to persistence in adult education among those who needed more than a year of adult education to achieve real gains in reading skills.

2. GED Receipt. While the relationship between postprogram test scores and months of adult education was not significantly affected by whether or not participation occurred under a mandate, the payoff in terms of GED receipt was. The second panel of Table 3.7 reports the predicted percentage of students receiving a GED during the follow-up period—among students with 1, 6, 12, and 18 total months of adult education—separately for individuals who were and

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[13] In particular, because welfare recipients with young children were administered a lengthy survey module regarding child outcomes, they were not administered the CASAS math test at the time of the follow-up interview.

National Evaluation of Welfare-to-Work Strategies

Table 3.6

For Sample Members Without a High School Diploma or GED at Random Assignment:[a] Predicted TALS Document Literacy Test Score, by Subgroup and Months of Participation in Adult Education

	Months of Participation			
Subgroup	1 Month	6 Months	12 Months	18 Months
Full Sample	251.15	256.65	250.91	270.71
Difference		5.50	-5.74	19.80
Effect size[b]		0.11	-0.12	0.40
Mandate				
HCD Group	253.36	255.61	255.92	274.06
Difference		2.25	0.31	18.14
Effect size[b]		0.05	0.01	0.37
Control Group	245.41	257.43	232.21	263.42
Difference		12.03	-25.22	31.20
Effect size[b]		0.25	-0.51	0.64
Site				
Atlanta	249.86	260.89	261.75	274.28
Difference		11.04	0.86	12.53
Effect size[b]		0.23	0.02	0.26
Grand Rapids	262.16	275.73	265.54	285.87
Difference		13.57	-10.19	20.33
Effect size[b]		0.28	-0.21	0.41
Riverside	234.90	224.36	229.19	265.98
Difference		-10.54	4.82	36.79
Effect size[b]		-0.22	0.10	0.75
Hours per Week				
Less than 20 hours per week	254.29	260.02	250.45	276.23
Difference		5.73	-9.57	25.79
Effect size[b]		0.12	-0.20	0.53
20 or more hours per week	240.77	241.38	251.94	251.17
Difference		0.61	10.56	-0.76
Effect size[b]		0.01	0.22	-0.02

(continued)

Table 3.6 (continued)

	Months of Participation			
Subgroup	1 Month	6 Months	12 Months	18 Months
Initial Academic Achievement				
"Normal" academic achievement	252.16	257.33	252.40	274.79
Difference		5.17	-4.93	22.39
Effect size[b]		0.11	-0.10	0.46
High initial CASAS math test score	209.56	214.41	208.17	226.61
Difference		4.85	-6.24	18.43
Effect size[b]		0.10	-0.13	0.38
High initial TALS literacy score	279.70	285.88	280.79	299.76
Difference		6.17	-5.08	18.97
Effect size[b]		0.13	-0.10	0.39
Highest Grade Completed				
Grade 8 or below	233.05	231.04	256.60	261.43
Difference		-2.01	25.56	4.83
Effect size[b]		-0.04	0.52	0.10
Grade 9-10	252.37	262.88	246.77	272.26
Difference		10.51	-16.11	25.48
Effect size[b]		0.21	-0.33	0.52
Grade 11	253.44	253.64	254.02	268.38
Difference		0.20	0.38	14.35
Effect size[b]		0.00	0.01	0.29
Sample size	1,955			

(continued)

Table 3.6 (continued)

SOURCES: MDRC calculations from information routinely collected by welfare staff, TALS document literacy test and CASAS math test data, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Adult education includes ABE, GED, ESL, and high school completion classes.

HCD = Human Capital Development.

CASAS = Comprehensive Adult Student Assessment System.

TALS = Test of Applied Literacy Skills.

Survey respondents were interviewed between month 25 and month 31, counting the month in which random assignment occurred as month 1.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally.

A two-tailed t-test was applied to coefficients listed above. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

Brackets around groups of coefficients within models indicate that an F-test of joint significance was applied to these variables. This statistic tests the hypothesis that the bracketed coefficients are statistically distinguishable from zero. Statistical significance levels are indicated as: † = 10 percent; †† = 5 percent; and ††† = 1 percent.

[a] The calculations in this table include only sample members who had both baseline and follow-up test score data in the Atlanta, Grand Rapids, and Riverside education-focused programs and had participated in adult education within 2 years of random assignment for more than 1 month but less than 21 months. Individuals who participated for 21 months or more were excluded due to small sample sizes.

[b] The effect size equals the difference divided by the standard deviation of the outcome measure for the control group. The standard deviation for control group members of the TALS document literacy test score is 49.

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Table 3.7

For Sample Members Without a High School Diploma or GED at Random Assignment:[a] Predicted Percentage of Students Receiving a GED, by Subgroup and Months of Participation in Adult Education

	Months of Participation			
Subgroup	1 Month	6 Months	12 Months	18 Months
Full Sample	8.07	28.97	21.29	12.59
Difference		20.90	-7.67	-8.70
Effect size[b]		0.87	-0.32	-0.36
Mandate				
HCD Group	10.27	25.63	23.91	††† 25.00
Difference		15.35	-1.72	1.10
Effect size[b]		0.64	-0.07	0.05
Control Group	2.27	29.81	0.00	0.00
Difference		27.54	-29.81	0.00
Effect size[b]		1.15	-1.24	0.00
Site				
Atlanta	15.16	49.78	45.62	††† 50.82
Difference		34.62	-4.17	5.20
Effect size[b]		1.44	-0.17	0.22
Grand Rapids	17.31	31.47	31.66	-3.53
Difference		14.16	0.19	-35.18
Effect size[b]		0.59	0.01	-1.47
Riverside	0.00	4.44	0.00	73.62
Difference		4.44	-4.44	73.62
Effect size[b]		0.19	-0.19	3.07
Hours per Week				
Less than 20 hours per week	12.57	21.45	18.26	†† 23.47
Difference		8.89	-3.20	5.22
Effect size[b]		0.37	-0.13	0.22
20 or more hours per week	0.00	30.77	22.66	32.36
Difference		30.77	-8.11	9.70
Effect size[b]		1.28	-0.34	0.40

(continued)

Table 3.7 (continued)

Subgroup	Months of Participation			
	1 Month	6 Months	12 Months	18 Months
Initial Academic Achievement				
				†††
"Normal" academic achievement	1.25	2.05	6.24	2.93
Difference		0.79	4.19	-3.31
Effect size[b]		0.03	0.17	-0.14
High initial CASAS math test score	20.00	52.11	39.33	36.87
Difference		32.11	-12.78	-2.46
Effect size[b]		1.34	-0.53	-0.10
High initial TALS literacy score	1.54	14.44	9.91	16.06
Difference		12.90	-4.53	6.16
Effect size[b]		0.54	-0.19	0.26
Highest Grade Completed				
				††
Grade 8 or below	0.00	1.12	0.00	0.24
Difference		1.12	-1.12	0.24
Effect size[b]		0.05	-0.05	0.01
Grade 9-10	12.87	23.60	16.63	27.83
Difference		10.73	-6.97	11.19
Effect size[b]		0.45	-0.29	0.47
Grade 11	10.20	28.87	36.49	12.37
Difference		18.67	7.62	-24.12
Effect size[b]		0.78	0.32	-1.01
Sample size	1,962			

(continued)

Table 3.7 (continued)

SOURCES: MDRC calculations from information routinely collected by welfare staff, TALS document literacy test and CASAS math test data, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Adult education includes ABE, GED, ESL, and high school completion classes.

HCD = Human Capital Development.

CASAS = Comprehensive Adult Student Assessment System.

TALS = Test of Applied Literacy Skills.

Survey respondents were interviewed between month 25 and month 31, counting the month in which random assignment occurred as month 1.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally.

A two-tailed t-test was applied to coefficients listed above. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

Brackets around groups of coefficients within models indicate that an F-test of joint significance was applied to these variables. This statistic tests the hypothesis that the bracketed coefficients are statistically distinguishable from zero. Statistical significance levels are indicated as: † = 10 percent; †† = 5 percent; and ††† = 1 percent.

[a] The calculations in this table include only sample members who had both baseline and follow-up test score data in the Atlanta, Grand Rapids, and Riverside education-focused programs and had participated in adult education within 2 years of random assignment for more than 1 month but less than 21 months. Individuals who participated for 21 months or more were excluded due to small sample sizes.

[b] The effect size equals the difference divided by the standard deviation of the outcome measure for the control group. The standard deviation for control group members of GED receipt is 24.

were not under a mandate to participate.[14] The statistical test reported in the table indicates that there was a statistically significant difference in the estimated payoff for the two research groups.

Furthermore, the estimates reported in the table suggest that the relationship between GED receipt and months spent in adult education during the follow-up period was somewhat stronger among individuals whose participation was voluntary. In particular, among those who were under a mandate, 10 percent of individuals with only one month of education received their GED by the end of the follow-up period, while the same can be said for only 2 percent of their counterparts in the control group. However, among welfare recipients who reported six months of education, 26 percent of the group that was under the mandate reported obtaining a GED during the follow-up period, compared with 30 percent of the group that was not. In other words, while the percentage of voluntary adult education participants receiving a GED during the first month of adult education lagged behind that of the group under the mandate, by six months into their participation in adult education this pattern was reversed. It is possible that those who chose to participate in adult education to get a GED had a greater need for additional education than those who were placed in adult education by a mandatory welfare-to-work program. It is likely that control group members who were close to passing the GED test just took it without formal preparation, while those in the program group were first enrolled in formal adult education programs.

D. Site-Specific Differences in the Payoff to Additional Adult Education

Another important question to address is whether there were systematic differences in the payoff to adult education across the three programs we studied. If such differences existed, they would warrant further exploration of variation in sample characteristics and program approaches across the sites.

The third panel of Table 3.6 shows predicted postprogram TALS document literacy test scores across individuals who reported receiving different amounts of adult education, separately for Atlanta, Grand Rapids, and Riverside. The observed patterns suggest that there were some differences in the payoff to adult education across these sites. First of all, sample members' test scores at the outset of their participation in adult education were substantially lower in Riverside than in the other sites. In particular, the predicted postprogram TALS scores among individuals in the Riverside site who reported only one month of adult education were approximately 15 points, or .31 of a standard deviation, lower than the scores of their counterparts in Atlanta, and about 27 points, or .55 of a standard deviation, lower than the scores of their counterparts in Grand Rapids. Moreover, in Riverside, the overall differences in postprogram test scores across individuals with 12 as opposed to 18 months of participation appear to be smaller than in the other two sites. However, the variation in patterns across the three sites was not statistically significant.

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[14] The predicted values in this table were based on a ordinary least squares regression. Predicted percentages were truncated to remain between zero and 100. Thus, the predicted values of zero that are observed in the table actually represent negative predicted values. While a logit or probit estimate would have accomplished this automatically, the data in this sample were not sufficient to generate these maximum likelihood estimates. Moreover, the nonlinear specification of the key independent variable reduces the likelihood of any bias in the estimates caused by restrictions of linearity.

The third panel of Table 3.7 reports site-specific estimates of the relationship between participation in adult education and GED receipt. The table suggests that there was statistically significant variation in this relationship across the three sites. As with postprogram literacy scores, the relationship between participation in adult education and GED receipt was stronger in Grand Rapids and Atlanta but weaker in Riverside. While this may indicate that education programs were stronger in Grand Rapids and Atlanta, it also could reflect differences in rules and policies used by the welfare-to-work programs.

E. Individual Characteristics and the Payoff to Time Spent in Adult Education

In addition to evaluating the effectiveness of adult education programs in general, an important goal of the NEWWS Evaluation is to assess to what extent the effectiveness of adult education programs differs across various subgroups. Tables 3.6 and 3.7 present predicted outcome levels for individuals who reported different amounts of adult education, within several subgroups defined using individual characteristics and program experiences. These estimates are based on regressions which allowed the payoff to adult education to vary across subgroups.

1. Hours per Week. Throughout this chapter, we have examined the relationship between academic achievement and the length of students' stay in adult education, without regard for the reported number of hours spent in class each week. At the end of the follow-up period, students who participated in adult education were asked to report the average number of hours dedicated to that activity each week. To the extent that the hours per week reflect the intensity of the educational experience or the degree of students' engagement, we might expect those who participated for more hours each week to experience a stronger payoff to the time they spent in adult education. In order to explore this question Tables 3.6 and 3.7 report predicted TALS and GED outcomes separately for those who reported spending 20 or more hours per week in adult education and those who reported attending less than 20 hours per week.[15]

With respect to postprogram TALS scores, the statistical test reported in Table 3.6 indicates that there was no statistically significant difference in the payoff estimated for these two groups. However, predicted outcome levels for individuals who reported receiving different amounts of adult education during the follow-up period do suggest some interesting patterns. In particular, students who attended classes for 20 or more hours per week began with lower scores and experienced earlier gains, which ceased to accrue after 12 months in the program. On the other hand, individuals who attended for less than 20 hours per week appeared to start with higher test scores and experienced relatively small benefits to their participation until after a year in the program.

With respect to GED receipt, the predicted outcome levels indicate that the positive relationship between length of stay in adult education and GED receipt is stronger for students who dedicate more hours to this activity each week. As might be expected, sample members who participated for less than 20 hours per week had a higher *initial* likelihood of receiving the credential but experienced a smaller payoff to additional participation. These differences were statistically significant.

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[15] Note that hours per week were measured retrospectively, at the two-year follow-up interview. Often, at least several months had passed between the last spell of participation in adult education and the interview. Therefore, it is possible that this variable was measured with considerable error. If so, it is possible that measurement error has biased these coefficient estimates toward zero.

2. Initial Academic Preparation. Among the important questions to consider is the extent to which benefits to participation in adult education vary with sample members' level of academic preparation. To the extent that benefits are concentrated among those with different levels of academic achievement or vary across students with different levels of formal education, programs may decide to target their adult education resources differently or adjust their expectations and time lines. To answer this question, we estimated several regression models which allowed the payoff to adult education to differ depending on individuals' initial education and academic achievement. The results suggest that initial academic preparation did not affect the relationship between months of adult education and postprogram literacy scores. However, as shown in Table 3.7, those with higher initial test scores were more likely to benefit from subsequent participation in adult education by obtaining a GED credential.

The final panel of Table 3.7 breaks down the relationship between participation in adult education and GED receipt for sample members with different levels of formal education. The results suggest a strong relationship between the payoff to adult education and the highest grade completed before entering adult education. In particular, individuals are divided into three groups, those who completed 8th grade or lower, those who completed 9th or 10th grade, and those who completed 11th grade. It appears that the relationship between adult education and GED receipt is stronger for those with who entered the study with more formal education. In particular, the predicted percentages of GED receipt among individuals with eight years or less education suggest that virtually none of them received their GED during the follow-up period and that the payoff to additional months of education in terms of GED receipt is extremely limited.[16] This is consistent with the finding from Chapter 2 that the impact of assignment to an education-focused program had a limited effect on GED receipt for individuals who started with an 8th grade education or less. Understandably, it appears to be difficult for an adult education program to make up for so many years of formal education, especially within a two-year follow-up period.

On the other hand, individuals who had completed 9 years or more of education at random assignment experienced a substantial payoff to participation in adult education in terms of their likelihood of receiving a GED. For example, among those who completed 9th or 10th grade before random assignment, 13 percent of individuals completing only 1 month of adult education received their GED during the follow-up period, while 24 percent of the individuals completing 6 months of adult education did the same. This difference was even larger for sample members who completed 11 years of formal education before participation in adult education. Moreover, while individuals with 9 or 10 years of formal education experienced a decline in GED receipt between 6 and 12 months of adult education, individuals who completed 11th grade experienced an increase from 29 to 36 percent over this same period of involvement.

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[16] Note that these predicted percentages are generated from a ordinary least squares regression, with the predicted values restricted to fall between zero and 100. To the extent that the actual predicted values were negative, this may slightly underestimate the payoff to additional months of education among this subgroup of students.

3. Barriers to Participation. Earlier evidence suggests the possibility that barriers to participation in adult education, such as having young children or having family or personal problems, might negatively affect participants' ability to benefit from their participation. For example, evidence presented in Chapter 2 indicates that random assignment to an education-focused welfare-to-work program had the smallest effect on participation in adult education among clients who reported having a high level of family or personal problems. In order to explore whether or not these factors limited the payoff to the time individuals spent in adult education, the relationship between months of education and GED receipt as well as postprogram TALS document literacy test scores were estimated separately for individuals with many family or personal problems and parental concerns and for individuals who had children younger than 6 in their household. The results indicate that there was no significant relationship between these barriers and the relationship between educational outcomes and additional months of adult education. Chapter 2 found that greater barriers to participation were associated with less favorable program effects on educational outcomes. Our findings in this analysis suggest that lower rates of participation might explain these differences, rather than smaller payoffs for those who did participate.

4. Attitudes Toward School. Another important issue to address is whether the relationship between time spent in adult education activities and postprogram test scores depends on students' attitudes toward school. To explore this hypothesis, we estimated the relationship between months of participation and postprogram educational outcomes, allowing it to vary according to students' disposition toward school. This was measured by whether students reported any participation in adult education activities over the past year and whether the students reported having a preference for school. If a student reported either of these, then that student was considered to have a positive attitude toward adult education. The estimates suggest that student attitudes toward adult education did not affect the payoff. Together with the findings reported in Chapter 2, this suggests that welfare-to-work programs can successfully overcome participants' initial distaste for adult education. Such lack of motivation did not affect program impacts on participation rates, and it apparently did not affect the payoff to participation either.

F. Provider Characteristics and the Payoff to Time Spent in Adult Education

The previous section examined whether the relationship between participation and education outcomes varied with participants' individual characteristics. This section explores to what extent the payoff from adult education varied across the schools and agencies that provided adult education services to sample members in our study.[17] Did certain types of providers improve educational outcomes more than others? Did teachers' experience and education matter?

1. Teachers' Experience. To explore the effects of teachers' experience on the payoff to adult education, we estimated the usual relationship between participation in adult education and educational outcomes, allowing it to vary with the average years of teaching experience of teachers at each adult education provider.[18] Tables 3.8 and 3.9 present predicted postprogram outcomes for individuals who received different amounts of adult education, estimated separately for different levels of experience among the teachers at each provider. In particular, we predicted educational outcomes for sample members who attended classes at hypothetical providers whose teachers had 15 as opposed to 25 years of experience. While these levels of experiences may seem somewhat high, it should be noted that the adult education providers surveyed employed relatively experienced teachers. In particular, the average level of teacher experience was approximately 17 years, while the standard deviation of teacher experience was about 7.[19]

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[17] Not all the individuals in the sample attended classes at providers who were surveyed as part of the teacher survey. Therefore, the sample of individuals upon which we base our estimates of the effect of provider characteristics on the payoff to adult education is different from the sample of individuals upon which we have based our estimates of the payoff to education thus far.

With respect to postprogram test scores, the statistical test reported in Table 3.8 indicates that the payoff to months of education was significantly greater in classes with more experienced teachers. For example, when the average level of experience was 25 years, the difference in test scores between respondents with one month and six months of adult education was predicted to be 9.07 points, or .19 of a standard deviation. When the average experience was 15 years, this payoff to five additional months of education dropped to 2.72 points, or .06 of a standard deviation.

In terms of GED receipt, on the other hand, the payoff to adult education seems to have been *negatively* related to teachers' experience. As shown in Table 3.9, at providers where the average teacher had 15 years of experience, the predicted difference in GED receipt between individuals with one and six months of adult education was 11 percentage points. However, additional months of education at providers with more experienced teachers (25 years on average) had no such positive effect. Although this difference is not statistically significant, the explanation for it is not obvious. One potential explanation is that teachers with different levels of experience have different teaching approaches, and that teaching approaches which promote success in obtaining a GED may be different from approaches which promote increases in measured literacy. It does not, however, suggest that students at providers who employ more experienced teachers experience universally strong payoffs to adult education.

2. Teachers' Education. Tables 3.8 and 3.9 also report predicted postprogram TALS scores and GED receipt among individuals with varying levels of adult education, separated according to the percentage of teachers at their providers who possessed a master's degree. The statistical test reported in Table 3.9 suggests that there was no significant relationship between the payoff to adult education in terms of GED receipt and the proportion of teachers who had a master's degree. While the predicted percentages in the table suggest that there might have been

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[18] The actual specification interacted average experience and average experience squared, so that the possibility of diminishing returns could be accounted for. Also, remember that all the estimates of the relationship between time spent in adult education and postprogram education outcomes include a series of dummy variables, one for each provider, in order to account for any unobserved provider-level variables that might have a direct effect on postprogram educational outcomes and might be correlated with time spent in adult education. Estimating these fixed effects avoids potential bias of the standard errors associated with the estimated effect of time spent in adult education. Unfortunately, including the fixed effects precludes us from estimating the *direct* effects of provider characteristics on student outcomes; instead, we only can estimate the relationship between provider characteristics and the payoff to time spent in adult education.

[19] More precisely, the average level of teacher experience *across individuals* in our sample—in other words, the average level of experience encountered by the *average student* at the provider—was 16.67 years, and the standard deviation was 6.67. Note also that the average years of experience includes all types of teaching experience, and does not necessarily equate to years of teaching at the adult level.

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Table 3.8

For Sample Members Without a High School Diploma or GED at Random Assignment:[a] Predicted TALS Document Literacy Test Score, by Provider Characteristics and Months of Participation

Characteristic	Months of Participation 1 Month	6 Months	12 Months	18 Months
Average Teaching Experience				
15 years	233.63	236.34	253.46	† 223.53
Difference		2.72	17.12	-29.94
Effect size[b]		0.06	0.35	-0.61
25 years	233.63	242.70	237.59	260.27
Difference		9.07	-5.11	22.69
Effect size[b]		0.19	-0.10	0.46
Teacher's Education				
25% with master's degree	236.92	236.96	245.85	† 239.42
Difference		0.04	8.90	-6.43
Effect size[b]		0.00	0.18	-0.13
50% with master's degree	236.92	236.11	239.16	248.30
Difference		-0.80	3.04	9.15
Effect size[b]		-0.02	0.06	0.19
75% with master's degree	236.92	235.27	232.47	257.19
Difference		-1.65	-2.81	24.72
Effect size[b]		-0.03	-0.06	0.50
Sample size	452			

(continued)

119

Table 3.8 (continued)

SOURCES: MDRC calculations from information routinely collected by welfare staff, TALS document literacy test and CASAS math test data, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Adult education includes ABE, GED, ESL, and high school completion classes.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

Survey respondents were interviewed between month 25 and month 31, counting the month in which random assignment occurred as month 1.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally.

A two-tailed t-test was applied to coefficients listed above. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

Brackets around groups of coefficients within models indicate that an F-test of joint significance was applied to these variables. This statistic tests the hypothesis that the bracketed coefficients are statistically distinguishable from zero. Statistical significance levels are indicated as: † = 10 percent; †† = 5 percent; and ††† = 1 percent.

[a] The calculations in this table include only sample members who had both baseline and follow-up test score data in the Atlanta, Grand Rapids, and Riverside education-focused programs and had participated in adult education within 2 years of random assignment for more than 1 month but less than 21 months. Individuals who participated for 21 months or more were excluded due to small sample sizes.

[b] The effect size equals the difference divided by the standard deviation of the outcome measure for the control group. The standard deviation for control group members of the TALS document literacy test score is 49.

Table 3.9

For Sample Members Without a High School Diploma or GED at Random Assignment:[a] Predicted Percentage of Students Receiving a GED, by Provider Characteristics and Months of Participation

	Months of Participation			
Characteristic	1 Month	6 Months	12 Months	18 Months
Average Teaching Experience				
15 years	23.85	34.60	36.02	12.48
Difference		10.76	1.41	-23.53
Effect size[b]		0.45	0.06	-0.98
25 years	23.85	19.01	22.09	9.36
Difference		-4.84	3.08	-12.73
Effect size[b]		-0.20	0.13	-0.53
Teacher's Education				
25% with master's degree	19.81	24.72	23.44	19.65
Difference		4.91	-1.27	-3.80
Effect size[b]		0.20	-0.05	-0.16
50% with master's degree	19.81	27.74	22.93	37.68
Difference		7.93	-4.81	14.75
Effect size[b]		0.33	-0.20	0.61
75% with master's degree	19.81	30.76	22.42	55.72
Difference		10.95	-8.34	33.30
Effect size[b]		0.46	-0.35	1.39
Sample size	360			

(continued)

Table 3.9 (continued)

SOURCES: MDRC calculations from information routinely collected by welfare staff, TALS document literacy test and CASAS math test data, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Adult education includes ABE, GED, ESL, and high school completion classes.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

Survey respondents were interviewed between month 25 and month 31, counting the month in which random assignment occurred as month 1.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally.

A two-tailed t-test was applied to coefficients listed above. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

Brackets around groups of coefficients within models indicate that an F-test of joint significance was applied to these variables. This statistic tests the hypothesis that the bracketed coefficients are statistically distinguishable from zero. Statistical significance levels are indicated as: † = 10 percent; †† = 5 percent; and ††† = 1 percent.

[a] The calculations in this table include only sample members who had both baseline and follow-up test score data in the Atlanta, Grand Rapids, and Riverside education-focused programs and had participated in adult education within 2 years of random assignment for more than 1 month but less than 21 months. Individuals who participated for 21 months or more were excluded due to small sample sizes.

[b] The effect size equals the difference divided by the standard deviation of the outcome measure for the control group. The standard deviation for control group members of GED receipt is 24.

slight differences in the payoff to the first six months of adult education, these differences are not statistically significant.

On the other hand, the evidence in Table 3.8 suggests that there *was* a relationship between the percentage of the teachers who possessed advanced degrees and the payoff to time spent in adult education in terms of literacy skills. The predicted outcome levels as well as the statistical test reported in this table indicate that the relationship between TALS scores and months of adult education was significantly enhanced by the percentage of the teachers who had a master's degree or higher. In the table we compare effects of additional months of education at two different hypothetical providers. Of the teaching staff at the first provider, it was assumed that 75 percent had advanced degrees, compared with 25 percent at the other provider. We then applied these numbers to the regression estimates and calculated the incremental gain in reading skills between 12 and 18 months of participation in adult education. We found that reading scores were not increased at the hypothetical provider with the less educated teaching staff (scores actually went down a little) and that scores increased by 25 points, or .50 of a standard deviation, at the other hypothetical provider.[20]

3. Other Provider Characteristics. Several other provider characteristics might be expected to influence the relationship between time spent in adult education and postprogram educational outcomes. To explore these, the JOBS teacher survey collected data on such factors as average class size, the degree of individual attention given to students, the proportion of staff who worked full time, the connection between the education provider and the JOBS program, the proportion of JOBS students in the classes at each provider, the instructional focus of the teachers, and the exit standards in place at each program. The distribution of these characteristics across providers was described and discussed in Chapter 2. Interestingly, teaching experience and education were the only characteristics that were significantly related to the payoff to time spent in adult education.

Taken at face value, the findings regarding provider characteristics suggest that teachers' qualifications are the primary provider-level factors which affect the payoff to adult education. In terms of literacy skills, students appear to benefit more when their time in adult education is spent with teachers who are both more experienced and more educated. Beyond this, other provider characteristics do not seem to be important.

It is possible, however, that this finding reflects a kind of "aggregation bias," or "measurement error." Because the provider characteristics used in these estimates were the average responses from the teachers who were interviewed at each provider, they thus may not have reflected students' actual experiences as accurately as other data might have. For example, if we had collected data regarding students' specific experiences, we might know the actual size of the class each student attended. Instead, we surveyed teachers, which only enabled us to measure a sense of the *average* class size at each provider. Therefore, we can say that class size was measured loosely, or with some error. If this sort of measurement error was present, the estimated relationship would likely be biased toward zero.[21] Therefore, to the extent that this phenomenon was present, we may have underestimated the effects of some provider characteristics on student outcomes.

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[20] Among the *individuals* in this sample, the mean proportion of teachers at their providers with an advanced degree is 52 percent, and the standard deviation is 25 percentage points.

VI. Conclusions

Overall, the evidence in this chapter suggests that, controlling for initial academic preparation, the amount of time welfare recipients spent in adult education programs was significantly and positively related to postprogram math achievement, literacy achievement, and GED receipt. Apparently, the magnitude of the effect depended on the outcome in question as well as on the number of months of adult education an individual had already accumulated. For example, whereas during the first 12 months additional months of participation in adult education were almost completely unrelated to postprogram reading scores, the predicted difference between the postprogram TALS scores of individuals with 12 as opposed to 18 months of participation was about 20 points, or .40 of a standard deviation. This suggests that long-term participation in adult education programs, as currently structured, is needed to materially improve the reading skills of participants in welfare-to-work programs.

For math scores, on the other hand, the payoff to additional months of adult education appears to have been concentrated during the first six months of participation. Specifically, the predicted difference between one and six months of participation was about 3.9 points, or .28 of a standard deviation, with little further gain associated with additional participation beyond that point. Therefore, to the extent that policymakers are interested in improving welfare recipients' math skills, they might expect improvement to occur soon after the recipients begin participating in adult education. However, inasmuch as this effect appears to flatten out after six to eight months, the ability to improve math skills further by these programs as currently structured appears limited. We cannot be sure that these relationships are causal. However, the delay in the effect of reading scores—combined with the fact that reading and math scores do not follow a similar pattern—undermines the theory that individuals with more months in adult education have higher postprogram test scores simply because they are more motivated.

One way to gain perspective on the magnitude of these "effects" is to compare them with the association between formal education and achievement among these individuals before their participation in adult education. To accomplish this, we estimated the association between the highest grade completed by sample members and their reading and math scores at the time of random assignment. An additional year of formal education is associated with 8.3 points, or .17 of a standard deviation, on the baseline TALS test. Assuming that sample members attended school for an average of 9 months per year, multiplying this by two years gives an amount of prior education comparable to 18 months in adult education. According to this comparison, 18 months of formal education among this group of people yielded an average of 16.6 points, or .34 of a standard deviation, on the TALS. Compared to the predicted .40 standard deviation difference associated with 18 months in adult education, this suggests that, among this group of individuals, the payoff to adult education is similar to the average payoff to the formal education they received before entering the program. Therefore, for the sample we studied, the literacy payoffs to formal education and adult education appear to be quite similar, or slightly larger than the payoffs associated with the years they spent in formal education.

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[21] Gujarati, 1995.

With respect to math skills, the regression estimates suggest that a year of formal education was associated with 1.46 points, or .10 of a standard deviation, on baseline math scores. Again assuming again that each year of formal education was associated with about 9 months in school, this suggests that 18 months of formal education would be associated with a difference of about .21 of a standard deviation in math skills. The predicted difference in math skills between 1 month and 18 months of adult education is 8.94 points, or .64 of a standard deviation[22]—*more than three times the payoff to respondents' prior formal education.*

The relationship between GED receipt and participation in adult education follows a more bell-shaped pattern during the first year and a half. There appears to be a positive association between months of adult education and the percentage of individuals receiving their GED during the first six months. Then, for the next six months, the relationship appears to flatten out and become negative. The estimates in Table 3.5 suggest that 8 percent of the individuals with one month of adult education obtain their GED within the two-year follow-up period. On the other hand, 29 percent of the welfare recipients with six months of adult education obtained their degree during the follow-up period—a difference of 21 percentage points. In proportional terms, however, we can say that *individuals with six months of adult education are 263 percent more likely to receive their GED than individuals with only a minimal amount of participation.*

The results presented in this chapter also show conclusively that, in terms of test scores, the payoff to adult education was unrelated to whether or not it occurred under a mandate. On the other hand, the relationship between GED receipt and months of participation in adult education was *positively* related to whether or not that participation occurred as part of a mandatory welfare-to-work program. Thus, this evidence does not support concerns that *mandated* participation in adult education will not yield improvements in educational attainment and achievement.

The evidence here suggests that, whereas all subgroups of welfare recipients in our sample improved their measured skills after participating in adult education, for certain subgroups there was no relationship between adult education and GED receipt. In particular, those who entered the program with less than an eighth grade education experienced no relationship between time spent in adult education and GED receipt during our two-year follow-up period. This is consistent with program effects presented in Chapter 2 and with the idea that these individuals need so much remediation that it is unlikely that short-term participation in adult education will help them obtain a GED. On the other hand, although the differences are not statistically significant, the estimates in this chapter suggest that, if anything, these individuals experienced a stronger relationship between time spent in adult education and postprogram *academic achievement.*

The evidence suggests that, to the extent that welfare-to-work programs are interested in achieving improvements in literacy and math skills among individuals who do not possess a high school diploma or GED, they may want to target referrals toward students with the least formal education. On the other hand, to the extent that they are interested in producing differences in educational attainment among this group, they might consider targeting instruction toward students with higher levels of formal education. Moreover, this pattern suggests that the approach taken, the expectations held, and the time allotted for completion of a program of adult education ought to vary according to an individual's initial preparation. The evidence also suggests that reported barriers to participation, such as family and personal problems or the presence of young children—although potential impediments to participation—are not significant impediments to an individual's ability to benefit from participation.

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[22] This difference was generated using the coefficients in Table 3.3, column 2. The predicted difference between 1 and 18 months = 5*.78 + 6*.35 + 6*.49 = 8.94.

At the level of the agencies that provide training to adult education clients, only average teacher experience and the proportion of teachers with advanced degrees at each provider appeared to be related to the payoff to additional time spent in adult education. In particular, students appeared to experience larger improvements in literacy scores when they attended classes at providers with more experienced and better educated teachers. This suggests that to help clients improve these skills, welfare programs should consider referring students to providers with more qualified and more experienced teachers. On the other hand, to the extent that they are interested primarily in helping clients to obtain educational credentials, a focus on teachers' educational qualifications might not make a great deal of difference.

However, these findings should be interpreted with some caution. The provider-level characteristics in this study were measured in a way that failed to capture the specific experiences of individual students. As a result, they may underestimate the effects of these factors on the payoff to time spent in adult education. To the extent that this is a concern, future research should attempt to gather data that more specifically capture the circumstances, experiences, and educational resources available to each student.

Chapter 4

Does the Low-Wage Labor Market Value Basic Education? Effects of GED Receipt and Literacy Gains on the Self-Sufficiency of Welfare Recipients

I. Introduction

The purpose of this chapter is to analyze how milestones and benchmarks used in adult education programs relate to subsequent employment and self-sufficiency outcomes. The previous chapters discussed how adult education programs being operated in the context of state welfare-to-work initiatives affect traditional education outcomes, such as the receipt of high school credentials and the improvement of basic reading and math skills. An important assumption underlying the use of these outcomes is that they are related to subsequent success in the labor market. It is assumed that welfare recipients who increase their educational attainment and improve their basic skills will be better able to secure and retain employment and will be offered higher wages, enabling them to leave public assistance and become self-sufficient.

There is an extensive literature to support these basic assumptions. For decades, labor economists and education researchers have documented how accumulated education, basic skills, and education credentials affect earnings and other employment outcomes.[1] Since the early 1990s, researchers have found that the importance of education and skills in the labor market has increased, which partly explains the growing earnings inequality in the U.S. labor market during the last two decades.[2] All this is of particular concern to welfare recipients, whose basic skills are known to be lower than those of workers in the general population,[3] and whose prospects in the labor market are limited by this lack of skills and the related lack of educational credentials and work experience.[4]

Within the welfare population, basic skills and education credentials are important predictors of the extent of recipients' long-term welfare dependency. Pavetti[5] and Bane and Ellwood[6] performed analyses of welfare dynamics over time and found that recipients of AFDC who had lower levels of education were predicted to remain on welfare for longer periods.

Given all this, welfare-to-work programs have often included education and training components to increase the basic skills of welfare recipients. Also, education was an important component of the federal Job Opportunities and Basic Skills Training (JOBS) program, the centerpiece of the 1988 Family Support Act. However, many welfare-to-work programs use other approaches, focusing not on the classroom but on quick job entry and developing of skills in the workplace. Gueron and Pauly[7] summarized evaluations of early state welfare programs using these different approaches. They compared "high-cost" and "low-cost" programs, with the former more likely to include education services. Their review did not find that high-cost programs were more effective than low-cost programs in terms of the employment outcomes of welfare recipients. Some high-cost, education-focused programs were quite successful, but so were some programs that were focused on quick job entry.

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[1] Mincer, 1974; Polachek and Siebert, 1993; Sum et al., 1995.
[2] Levy and Murnane, 1992; Burtless, 1994.
[3] Kirsch et al., 1993; Zill et al., 1991.
[4] Burtless, 1997; Olson and Pavetti, 1996.
[5] Pavetti, 1993.
[6] Bane and Ellwood, 1994.

Another important study in this regard, the evaluation of California's Greater Avenues for Independence (GAIN) program, also failed to bolster Human Capital Development (HCD) programs as a promising way to help welfare recipients become self-sufficient.[8] Of the six California counties included in the study, one was far more successful in operating its GAIN program than the others. Compared with the other five counties in the study, Riverside County ran a program that was focused more on Labor Force Attachment and that placed less value on adult education and attainment of an education credential.

Within the GAIN evaluation, a special study was conducted of the effects of GAIN on those without high school credentials or otherwise deemed "in need of basic education." This study found that GAIN increased the basic skills of these welfare recipients in San Diego but not in the other counties.[9] It found significant impacts on GED receipt in four out of five of the counties. However, within the two-year follow-up period of this study, earnings and welfare impacts did not parallel the impacts on educational attainment and basic skills. Later nonexperimental work by Friedlander et al. failed to find such a relationship even in the third year of follow-up.[10]

The latest contribution to our understanding of HCD programs for welfare recipients is the National Evaluation of Welfare-to-Work Strategies (NEWWS)—the study of which this report is a part. As will be discussed in more detail in a later chapter, the NEWWS research design used random assignment to create experimental distinctions between two program approaches like those distinguished by Gueron and Pauly: Human Capital Development and Labor Force Attachment (LFA). Using random assignment both shields comparisons of these approaches from cross-site variation in program operation, welfare rules, and the social and economic environment and leaves the program approach as the only remaining factor that accounts for variation in program effects. Two-year findings from the NEWWS Evaluation were presented by Freedman et al., who enhanced the two-dimensional HCD-LFA picture by introducing other important dimensions, such as the programs' levels of enforcement.[11] Again, LFA programs appeared to be more successful than HCD programs, even in assisting welfare recipients who lacked a high school credential.[12]

Thus, the literature offers a somewhat confusing picture. On the one hand, researchers consistently find that basic skills and education credentials are important in the labor market, that persons who lack these assets are more likely to be poor and dependent on welfare, and that welfare recipients with lower basic skills remain on welfare longer. However, programs that directly address these shortcomings in clients' human capital are no more successful than programs that do not and that focus on quick job entry instead. What might explain this discrepancy?

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[7] Gueron and Pauly, 1991.

[8] Riccio et al., 1994.

[9] Martinson and Friedlander, 1994.

[10] Friedlander et al., 1994.

[11] Freedman et al., 1999.

[12] Across the board, the most successful program studied was the JOBS program in Portland, Oregon, which offered a more varied approach, combining elements from traditional LFA and HCD programs. However, it is difficult to separate the benefits of this approach from other characteristics of the Portland program and its local environment (see Scrivener et al., 1998, pp.13-43, for an extensive discussion of the features of this program).

Four important issues play a role in this discussion—all of which have been raised in earlier chapters in this report. First, the "human capital" literature focuses on skills and education accumulated during a lifetime, while the welfare program evaluation literature focuses on the effects of relatively short-term and low-intensity programs that are remedial in nature. A credential obtained in one context may not be interchangeable with a credential obtained in the other.

Second, the HCD programs studied were mandatory in nature, and the adult education services provided in them were usually not targeted specifically at welfare recipients. Chapter 2 of this report showed that many welfare recipients in the NEWWS programs did not want to go to school to study basic reading or math despite their lacking a high school credential and their low basic skill levels. Therefore, the evaluation literature measures the effects of education and training programs that are mandatory, unpopular, often not designed for the needs of welfare recipients, and unable to retain most students long enough to effect real change in their outcomes.

Third, all evaluations of education and training programs, whether welfare-related or not, have follow-up periods that are quite short. Combined with the low intensity and short duration of the programs themselves, this makes it difficult to identify program effects if they occur, especially as they affect more long-term outcomes such as job retention, earnings growth, and sustained self-sufficiency.

And fourth, the "human capital" literature may to some extent capture systematic underlying differences in the characteristics and backgrounds of different populations with different levels of educational attainment.

II. Summary of the Findings

- **Our analyses suggest that receipt of a GED improves the earnings of those assigned to participate in welfare-to-work programs. The estimated effect of a GED on annual earnings three years after random assignment was approximately $771. GED recipients also were predicted to receive fewer welfare benefits.**

Simple regression analyses suggest that welfare recipients benefit from receiving a GED certificate. This finding is fairly robust, holding up across different samples and different specifications of the analytical models. However, we were unable to confirm this finding using advanced statistical methods. Consequently, this finding remains tentative, and there is always a possibility that it is affected by model misspecification or selection bias.

- **Estimated effects of the GED were unaffected by the introduction of participation variables or reading test scores into the regression model. This suggests that our estimates capture the "credential effect" of the GED, rather than underlying skills or participation in adult education.**

Apparent effects of educational credentials on earnings and welfare receipt can represent different aspects of sample members' human capital. On the one hand, it is possible that such credentials "certify" individuals' participation in basic education or mastery of basic skills. On the other hand, the effect of the credential could be more superficial, enhancing the holders' ability to signal her persistence, seriousness, or employability. Our analyses suggest that the latter is the primary function of the GED for welfare recipients. Controlling for participation in education or test scores does not reduce estimated effects of the credential, as might be expected if the credential were simply a proxy for skills or classes taken.

- **The benefits of having a GED were greater when the credential was earned as part of a larger welfare-to-work program.**

Program group members who received a GED appeared to benefit more from this credential than did control group members. This is probably because after receiving the credential program group members would be more likely to proceed with a structured job search or with additional education or training, as mandated (and supported) by the welfare-to-work program.

- **Greater reading skill levels were associated with higher earnings, regardless of GED receipt.**

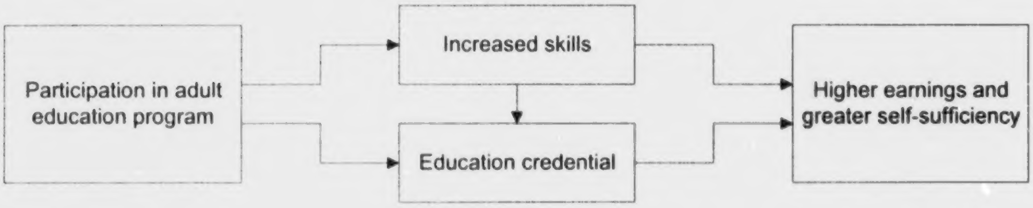
In analyses linking reading skills to earnings and welfare receipt, an increase of one standard deviation in TALS scores (measured at a two-year interview) was associated with $354 in additional earnings; however, there was no concomitant reduction in welfare receipt a year later.

III. The Value of Mediating Outcomes

One way to assess the effectiveness of HCD programs in general, and of adult education programs in particular, is to focus on these programs' effects on more immediate benchmarks and milestones. In this report, such analyses were done in Chapters 2 and 3, which showed impacts on reading and math scores and on GED receipt. In the relationship between adult education and improved employment outcomes these education outcomes operate as *mediating variables*. That is, effects of adult education programs on earnings and self-sufficiency are supposed to occur in a linear process like the path shown in Figure 4.1.

Figure 4.1

Hypothesized Relationship Between Adult Education and Labor Market Outcomes


Participation in adult education program
Increased skills
Education credential
Higher earnings and greater self-sufficiency

The assumptions underlying HCD programs do not identify other ways in which program participation on the left side of the figure affects the positive change in employment outcomes on the right side of the figure. (There is the possibility of a direct *negative* effect, inasmuch as some adult education participants may temporarily forgo employment opportunities.) Therefore, if Figure 4.1 is a valid depiction of reality, it should be possible to evaluate the effectiveness of these programs by concentrating on the boxes in the middle of the figure. This is both more convenient and more appropriate, because these outcomes can easily be collected by program administrators and do not suffer from the kind of delayed response affecting the link between participation and employment outcomes. However, this course of action raises a significant problem: its appropriateness depends entirely on the extent to which the mediating outcomes in the center of our model are truly predictive of the outcomes shown on the right.

Another way to think about this is to break down the effectiveness of adult education programs for welfare recipients into two parts. The first step concerns the programs' ability to improve the mediating outcomes. This was discussed in the two preceding chapters. The second step concerns the extent to which improvements in these mediators actually translate into better employment outcomes and increased self-sufficiency. In other words, do the outcomes targeted by adult education programs make a real difference in welfare recipients' ability to work and leave welfare?

The literature focusing on these relationships in the context of adult education programs for welfare recipients is quite limited. Probably the most studied outcome of these programs is receipt of the General Educational Development (GED) credential. However, most GED studies have examined the effects of this credential for the larger population of GED holders, not specifically as an outcome of adult education programs. In probably the most widely cited study of the labor market value of the GED, Cameron and Heckman found no significant difference in wages between GED holders and high school dropouts who did not have GEDs.[13] (Their findings were based on data from the National Longitudinal Study of Youth, or NLSY.) The only positive effect of GED receipt that they found is an increase in college attendance, but they noted that the college dropout rate was much higher for GED holders than for college students with regular high school diplomas.

Murnane et al. also used NLSY data to produce inferences about the labor market value of the GED and to describe how receipt of a GED affected subsequent participation in vocational training, college, and the military.[14] In their 1994 paper, Murnane et al. found evidence that among dropouts GED recipients were more likely to enter vocational training or college. They speculated that access to vocational training would provide a mechanism through which receipt of a GED could have a long-term and lasting effect on the labor market success of GED recipients (the "gatekeeper" effect). Some support for this hypothesis was presented in a subsequent paper,[15] which found that the long-run earnings trajectory of GED recipients was changed by their receipt of the credential, despite short-term reductions in their earnings and labor force participation as they pursued further education and training.

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[13] Cameron and Heckman, 1993.

[14] Murnane et al., 1994, 1995.

[15] Murnane et al., 1995.

A problem with these national studies based on NLSY data is that they use heterogeneous samples of high school dropouts to make comparisons between GED holders and those who do not have the credential. Many dropouts who do not pursue a GED may have decided against doing so because they are already employed or may think or know that they have good employment opportunities without a high school credential (because of a family business, for example). Therefore these findings may not be relevant for welfare recipients mandated to participate in adult education because of their failure to complete high school. The relevant question for adult education programs serving these welfare recipients is whether the GED is a meaningful milestone for *them*, not whether it improves the earnings prospects of high school dropouts in general.

An exception to these studies using national data was the analysis of the effects of GED receipt on earnings conducted by Quint et al. in their evaluation of the New Chance program.[16] New Chance, a demonstration program developed by MDRC in the mid-1980s, was targeted exclusively at young teen mothers who were receiving welfare and had dropped out of high school. The program increased substantially the number of these young women who held a GED, but it failed to produce significant earnings gains or reductions in welfare receipt. The authors approached this discrepancy by looking more closely at the earnings trajectories of sample members who received a GED. They found that GED receipt without subsequent training did not lead to better employment outcomes but that GED receipt followed by training did. They also found that program participants who attended basic education classes without completing them with a GED actually suffered earnings *losses*, which, in the average, offset the gains for those who did complete. However, it is unclear to what extent these findings from the New Chance program hold up in the larger welfare-to-work context. New Chance was limited to teen parents, was voluntary, and included young women who were highly motivated but also experienced a multitude of barriers to program participation and subsequent employment. It is unclear to what extent findings from this small-scale demonstration project can be generalized to the larger world of mandatory welfare-to-work programs.

While many studies have been done of the effects of obtaining education credentials, many fewer researchers have looked at the effects of improvements in basic skills. Mostly this is because it is quite difficult (and expensive) to measure changes in basic skills, especially in studies that involve a control group not subject to the program intervention. Among the exceptions are the New Chance demonstration[17] and the evaluation of California's GAIN program.[18] The New Chance demonstration found no program effects on literacy as measured with the Test for Adult Basic Education (TABE). Quint et al. did not explore the relationship between TABE scores and employment outcomes. In the GAIN evaluation, Martinson and Friedlander examined the impacts of California's GAIN program on TALS reading scores.[19] They found no impacts in most of the counties studied, but significant gains occurred in San Diego County. In their discussion of these findings they argued that the intensity of participation in basic education had probably not been large enough to produce consistently significant findings. Martinson and Friedlander did not study the relationship between test score gains in San Diego and subsequent employment and welfare outcomes there.

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[16] Quint et al., 1997.

[17] Quint et al., 1997; Quint et al., 1994.

[18] Martinson and Friedlander, 1994; Riccio et al., 1994.

[19] Martinson and Friedlander, 1994. For detailed descriptions of the TALS and CASAS tests, please refer to Appendix D.

IV. About the Analyses Presented Here

The purpose of the analyses presented in this chapter is to expand what we know about the effects of GED receipt and basic education for welfare recipients. Unlike the larger programs assessed in the main NEWWS Evaluation, these relationships cannot be studied directly using random assignment. It is simply not possible to "assign" individuals a certain number of hours of participation, to "assign" them a GED, or to randomly give them certain basic skill levels. Consequently, it is necessary to employ nonexperimental research methods to identify these relationships. In this regard, this study of adult education programs for welfare recipients is no different from most of the nonexperimental research discussed above. However, the NEWWS data have several important advantages over other available data, mostly pertaining to the relevance of the findings in a welfare-to-work context and to the availability of appropriate control variables.

The data collected for the NEWWS Evaluation offer an excellent opportunity to study the effects of adult education and GED receipt in a welfare-to-work context. The data used here cover a wide range of different welfare-to-work programs in different economic and program environments—increasing the relevance and representativeness of conclusions based on these data. In its focus on welfare recipients in mandatory programs, this study answers fundamentally different questions than much of the research cited above. Those studies described the effects of the GED for school dropouts in general, whereas the NEWWS data document those effects for dropouts who are welfare recipients in particular. Although this sample may be more disadvantaged, it has access to a more comprehensive set of services, and it faces a mandate to participate. All this could affect how adult education and GED receipt impact employment outcomes among welfare recipients.

A second advantage of the NEWWS data is the rich array of control variables available. Two sets of variables collected at program intake are rarely available in other data sources. The first is a number of variables obtained from the Private Opinion Survey (POS). This survey, administered before sample members knew whether or not they would be enrolled in the local JOBS program, captured sample members' opinions, preferences, and perceived barriers to participation and employment. Having these variables allows us to hold constant sample members' motivation and their readiness to participate in adult education, to pursue an education credential, to look for work, and to leave public assistance. The elusive concepts of motivation and work-readiness—captured by this baseline survey—play an important role in the process of self-selection that usually distinguishes GED recipients from nonrecipients, high school graduates from dropouts, and employed people from those who are not working. In most cross-sectional analyses (and, indeed, most time-series analyses as well) these concepts are difficult to control for. This means that estimates of effects of the GED, adult education, and other predictors of labor market success can be confounded with this uncontrolled self-selection process, exposing those estimates to what is commonly known as "selection bias."

Such bias can operate in various ways. Usually, it is assumed that if motivation and participation are confounded, estimates of program effects will be biased upward, that is, will seem larger than they really are. However, as was suggested above, the selection process does not always operate this way. Sometimes, motivation to participate in adult education or to pursue a GED may signal *negative* selection bias—for example, when a perceived lack of opportunity in the labor market prompts school dropouts to try to earn an alternative credential. Because this implies that GED recipients may sometimes have lower initial employment opportunities than their non-GED counterparts, estimates of the effect of the credential could be biased downward.

Either way, inclusion of variables measuring motivation and work-readiness should substantially minimize the extent of these selection biases. This will improve the quality of the estimates obtained, both in terms of their size and direction and in terms of their statistical precision.

Another set of control variables available in the NEWWS data is a group of variables measuring sample members' literacy and math skills before entry into a program. Many other data sources that are used to assess effects of adult education and GED receipt do not have such variables, adding another potential source of selection bias to estimates based on these data. Again, the usual assumption is that such a bias would increase the apparent effects of adult education and GED receipt, because dropouts with higher initial achievement levels are more likely to pass a GED test. However, in this case, selection may work backwards as well. School dropouts with lower literacy levels may have more difficulty finding employment *without* remedial education or a GED certificate, prompting them to enroll in adult education and pursue such a credential. Thus, not controlling for initial achievement levels also could produce either over- or underestimates of the effects of adult education and the GED. In seven of the eleven programs studied, the NEWWS data allow us to control for initial literacy and math scores, resolving this problem.

A final advantage of the NEWWS data is that we may use unbiased experimental impacts on both employment outcomes and explanatory variables to check the consistency of our findings with the unbiased experimental impact story told elsewhere in this and other NEWWS reports. Impacts on GED receipt and test scores were presented in Chapter 2, and impacts on employment, earnings, and welfare receipt were presented in previous NEWWS reports (for example, Freedman et al., 1999). Consistent patterns among these experimental estimates would suggest that they might be related. This would help support findings of similar relationships produced with less reliable nonexperimental methods.

In the remainder of this chapter, we will present effects of GED receipt and educational achievement gains on employment and welfare receipt of NEWWS sample members who did not have a high school diploma or GED at random assignment.[20] Details on the sample composition and sample sizes are provided in Appendix A.

Table 4.1 describes the samples featured in this chapter in some detail, showing demographic characteristics for sample members in all 11 NEWWS programs.[21] The table shows char-

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[20] Note that some sample members in Riverside, California, did have a high school diploma or GED but were deemed "in need of basic education" because of their low initial achievement levels as measured with the CASAS test of basic skills. These sample members are excluded from analyses of the effects of GED receipt, but they are included in other analyses.

[21] These and other statistics presented in this chapter were weighted so that each of the seven NEWWS sites is given equal representation regardless of the actual size of the study sample in that site.

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Table 4.1

For Sample Members Without a High School Diploma or GED at Random Assignment: Selected Characteristics of Sample Members, Pooled Across 11 Programs, by High School Diploma or GED Status at Follow-Up

Characteristic	Received High School Diploma or GED at Follow-Up[a]	Did Not Receive High School Diploma or GED by Follow-Up[a]	p-Value
Demographic Characteristics			
Gender (%)			
Male	1.3	3.8 ***	0.001
Female	98.8	96.2 ***	0.001
Age (%)			
Less than 19	16.2	4.0 ***	0.001
19-24	31.5	21.5 ***	0.001
25-34	37.4	46.2 ***	0.001
35-44	14.5	22.1 ***	0.001
45 and over	0.4	6.2 ***	0.001
Average age (years)	26.2	30.8 ***	0.000
Ethnicity (%)			
White	51.0	41.4 ***	0.001
Hispanic	6.1	8.7 ***	0.001
Black	40.0	47.2 ***	0.001
Black Hispanic	0.8	0.1 **	0.001
Native American/Alaskan Native	1.3	1.8 **	0.038
Asian/Pacific Islander	0.5	0.7	0.224
Other	0.4	0.1 **	0.001
Family Status			
Marital status (%)			
Never married	64.1	50.9 ***	0.001
Married, living with spouse	1.5	4.4 ***	0.001
Separated	22.5	25.0 ***	0.003
Divorced	11.8	18.4 ***	0.001
Widowed	0.1	1.3 ***	0.001
Number of children (%)			
1	44.7	38.3 ***	0.001
2	31.5	30.4	0.233
3 or more	23.9	31.4 ***	0.001
Average number of children	2.0	2.1 ***	0.000
Has any child age 0-5	80.7	54.7 ***	0.001
Has any child age 6-11	46.6	51.6 ***	0.001
Has any child age 12-18	19.7	38.8 ***	0.001

(continued)

Table 4.1 (continued)

Characteristic	Received High School Diploma or GED at Follow-Up[a]	Did Not Receive High School Diploma or GED by Follow-Up[a]	p-Value
Age of youngest child (%)			
2 or under	45.0	23.1 ***	0.001
3 to 5	35.7	31.5 ***	0.001
6 or over	19.3	45.3 ***	0.001
Had a child as a teenager	61.4	50.2 ***	0.001
Labor Force Status			
Worked full time for 6 months or more for one employer (%)	48.4	52.8 ***	0.001
Any earnings in past 12 months (%)	32.1	30.7	0.113
Currently employed (%)	6.4	7.2	0.149
Education Status			
Average highest grade completed	10.2	10.0 **	0.028
Enrolled in education or training in past 12 months (%)	34.6	17.8 ***	0.001
Currently enrolled in education or training (%)	37.1	12.5 ***	0.001
Public Assistance Status			
Total prior AFDC receipt[b] (%)			
None	14.4	9.0 ***	0.001
Less than 1 year	4.4	7.0 ***	0.001
1 year or more but less than 2 years	12.5	9.1 ***	0.001
2 years or more but less than 5 years	28.6	26.7 **	0.030
5 years or more but less than 10 years	24.0	20.7 ***	0.001
10 years or more	7.2	20.5 ***	0.001
Raised as a child in a household receiving AFDC (%)	34.1	33.5	0.509
First spell of AFDC receipt[c] (%)	20.3	16.3 ***	0.001
Housing Status			
Current housing status (%)			
Public housing	5.0	12.2 ***	0.001
Subsidized housing	14.1	13.4 ***	0.001
Emergency or temporary housing	5.2	2.7 ***	0.001
None of the above	76.6	73.0	0.268

(continued)

Table 4.1 (continued)

Characteristic	Received High School Diploma or GED at Follow-Up[a]	Did Not Receive High School Diploma or GED by Follow-Up[a]	p-Value
Site			
Atlanta	5.0	15.7 ***	0.001
Columbus	13.0	15.0 ***	0.006
Detroit	23.0	14.1 ***	0.001
Grand Rapids	15.8	14.7	0.145
Oklahoma	18.1	14.5 ***	0.001
Portland	18.2	14.5 ***	0.001
Riverside	4.4	9.2 ***	0.001
Sample size	280	3,994	

SOURCE: MDRC calculations from information routinely collected by welfare staff.

NOTES: To compensate for differences in sample members' likelihood of being chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally in the pooled estimates.

Distributions may not add to 100 percent because of rounding.

[a] The GED credential is given to those who pass the GED test and is intended to signify knowledge of high school subjects.

[b] This refers to the total number of months accumulated from one spell or more on an individual's own or spouse's AFDC case. It does not include AFDC receipt under a parent's name.

[c] This does not mean that such individuals are new to the AFDC rolls, only that this is their first spell on AFDC. This spell, however, may have lasted several years.

acteristics for program group members and control group members combined. It breaks down these characteristics by educational attainment status at follow-up, showing how those who received a credential differed from those who did not. Asterisks and p-values in the table show whether differences across the two groups are statistically significant. In general, it appears that GED recipients and those who received a high school diploma were younger than those who did not receive an education credential. Those with a credential were significantly more likely to be younger than 25, were more likely to be never married, were much more likely to have young children (more than 80 percent had a child under 6), but were somewhat less likely to have been employed full time for six months. All these differences suggest that GED recipients were less work-ready than their counterparts who did not get such a credential. On the other hand, GED recipients had less long-term welfare dependency, were more likely to have been enrolled in education or training programs in the past year, and were less likely to be living in public housing.

V. Data Sources and Outcome Measures

In our analyses, we combined a number of different data sources. Our primary explanatory variables (GED receipt, literacy, and math skills) were measured using a two-year follow-up survey conducted in each of the seven NEWWS sites.[22] This survey was conducted long enough after random assignment that most sample members would have completed their participation in adult education programs and most GED recipients would have obtained that credential by then. However, two years would have been too short to measure the effects of participation in education and GED receipt on sample members' employment outcomes and self-sufficiency. Therefore, we used three-year data from unemployment insurance earnings records and state or county welfare records to measure those effects. To keep them simple, our analyses are usually limited to five distinct outcomes:

- total earnings in year 3
- increase in earnings from year 2 to year 3
- average earnings per quarter employed in year 3
- number of months receiving AFDC in year 3
- change from year 2 to year 3 in the number of months receiving AFDC

In addition to the outcome variables and the explanatory variables from the survey, the analyses use control variables from baseline enrollment forms, the Private Opinion Survey conducted at baseline in four of the sites, baseline achievement tests, and administrative data covering the year before random assignment.

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[22] As indicated earlier, reading and math tests were conducted in only three of the seven sites.

VI. The Value of the GED: Evidence from Experimental Comparisons

As mentioned above, a rudimentary way to examine the relationship between GED receipt and employment and welfare outcomes is to compare experimental impacts on these outcomes for various sites and programs. The question is whether patterns of these impacts would support the hypothesis that GED receipt improves employment and welfare outcomes. (That is, were strong impacts on GED receipt observed for the same programs for which we found strong impacts on earnings or welfare receipt?)

Figure 4.2 shows fully experimental impact estimates for each of the 11 programs studied in the NEWWS Evaluation.[23] Impacts on GED receipt are shown on the horizontal axis, and impacts on year 3 earnings and year 3 AFDC receipt (in dollars) are shown on the vertical axis. (The earnings impacts are marked with diamonds; the AFDC impacts, with circles.) Looking at the earnings impacts, it seems that, with the exception of the Grand Rapids LFA program (the outlier on the left) and the Traditional program in Columbus (the only negative earnings impact), there is a modest positive relationship between impacts on GED receipt and impacts on earnings in the 11 programs studied. That is, the pattern of earnings impacts slopes up to the right of the graph.

However, such a relationship does not seem to exist in the AFDC impacts (all of which were negative, as shown in the graph). The reason for this may be that two opposing processes account for the impacts on AFDC receipt. On the one hand, AFDC payments could go down because of sanctions for nonparticipation—an effect that would probably be larger in programs with smaller impacts on GED receipt. On the other hand, however, one would expect AFDC impacts to follow earnings impacts, showing lower payments for sample members with higher earnings. This would result in larger AFDC reductions for programs with *larger* impacts on GED receipt. Clearly, Figure 4.2 does not allow us to distinguish between these two explanations.

VII. The Value of the GED: Ordinary Least Squares Regression Analysis

The next step in our analysis was to compare the earnings, earnings growth, and welfare receipt of GED recipients directly with the outcomes for sample members who did not receive a GED. To do this, we use a straightforward statistical procedure called *ordinary least squares (OLS) regression analysis.* In such a procedure, the effects of explanatory variables on outcome variables are measured by examining their relationship while simultaneously holding constant many other determinants of the outcome variables. This is done by introducing measures of those other determinants as "control variables" in the analysis. Common examples of such control variables are age, household composition, and prior work experience (see box).

As indicated above, however, the NEWWS data cover much more than the usual set of control variables, extending to important concepts such as motivation, readiness for school or work, literacy, and math skills. The analysis also includes a regular JOBS variable, which captures other aspects of the JOBS program besides the explanatory variables of interest. In the absence of such a variable, effects of such program components as case management, sanctions,

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[23] Remember, these are Atlanta HCD, Atlanta LFA, Columbus Integrated Case Management, Columbus Traditional Case Management, Detroit, Grand Rapids HCD, Grand Rapids LFA, Oklahoma City, Portland Oregon, Riverside HCD, and Riverside LFA.

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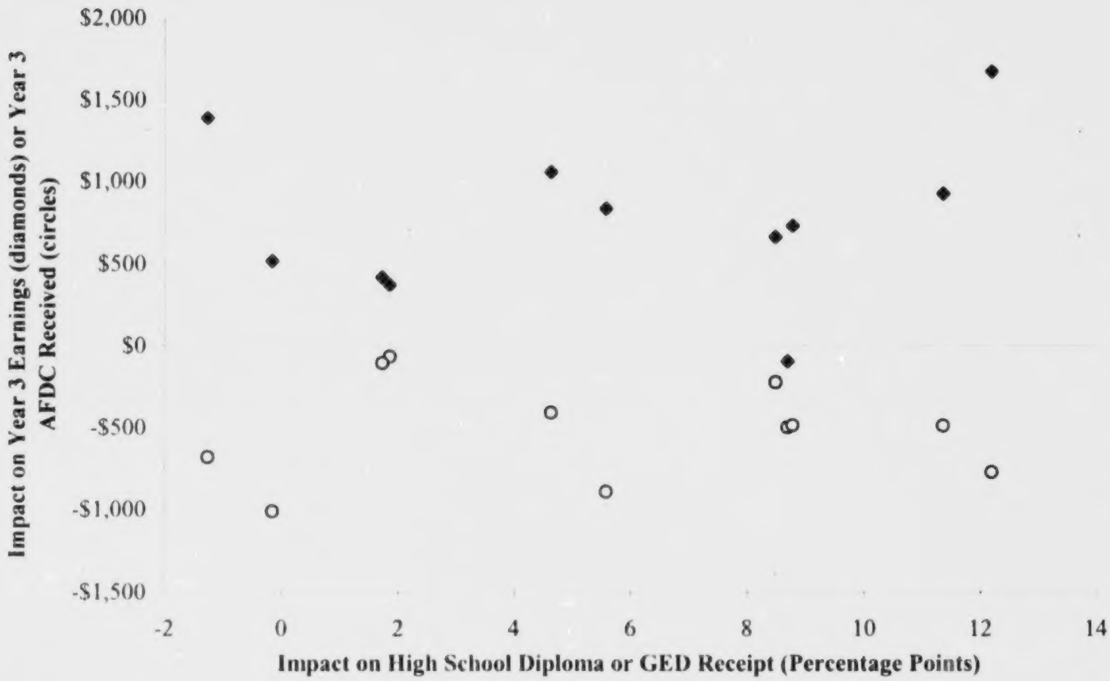
Figure 4.2

For Sample Members Without a High School Diploma or GED at Random Assignment: Program Impacts on Earnings and AFDC Receipt in Year 3, by Impact on High School Diploma or GED Status After Two Years



SOURCES: MDRC calculations from unemployment insurance (UI) earnings records, AFDC records, and the JOBS Two-Year Client Survey.

NOTES: Estimates were regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members.

To compensate for differences in sample members' likelihood of being chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally in the pooled estimates.

and job search assistance could be attributed inadvertently to the education variables that are of interest to us.

A. The Basic Model

Table 4.2 presents ordinary least squares (OLS) estimates of the effects of getting a GED (during the two-year follow-up period) on selected employment, earnings, and welfare outcomes in year 3. These are "average" effects for all sample members in the study, regardless of their research group status. The table shows that receiving a GED had substantial positive effects on welfare recipients' earnings, increasing those earnings by $836 on average in the third year of follow-up. This represents an increase of 30.5 percent of the mean for this outcome for the full sample.

The table also shows that this increase is accompanied by a substantial positive impact on sample members' earnings growth since the second year of follow-up. The mean difference in

Standard Control Variables

Analyses featured in this chapter always use the same set of basic control variables. The purpose of these variables is to isolate the effects of GED receipt, participation in adult education, and variation in basic skill levels even if those are correlated with other variables, such as demographics, motivation, or welfare history. The list of standard control variables follows:

Site (city, state)
Assignment to Job Opportunities and Basic Skills Training (JOBS) program
Marital status
Number of children
Any child older than 5
Ethnic group
Age
Gender
Employed in prior year/prior quarter
Earnings in prior year (also squared)/prior quarter
Received AFDC/Food Stamps in prior year/prior quarter
Number of months received AFDC/Food Stamps in prior year
Average amount of AFDC/Food Stamps per month in prior year
Parental concerns about going to work or school
Preference for school
Extent of family/personal problems
Feelings of depression
Feelings of mastery over life events

Table 4.2

For Sample Members Without a High School Diploma or GED at Random Assignment: Estimated Effects of Receiving a GED During Years 1-2 on Selected Year 3 Outcomes, Pooled Across 11 Programs

Outcome	Mean Outcome	Estimated Effect of Receiving a GED[a]	p-Value	R^2
Earnings in year 3 ($)	2,741	+836 ***	0.001	0.152
Increase in earnings relative to year 2 ($)	546	+797 ***	0.000	0.036
Earnings per quarter worked in year 3[b] ($)	1,615	+128	0.179	0.169
Total AFDC received in year 3 ($)	2,620	-237 *	0.059	0.298
Number of months receiving AFDC	6.50	-0.13	0.640	0.174
Change in number of months receiving AFDC, compared to year 2	-1.36	-0.73 ***	0.001	0.034
Sample size	4,274			

SOURCES: MDRC calculations from unemployment insurance (UI) earnings records, AFDC records, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: To compensate for differences in sample members' likelihood of being chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally in the pooled estimates.

Results presented in this table were obtained from ordinary least squares regression analyses, controlling for baseline characteristics, research group (program or control group), and barriers and attitudes measured with the Private Opinion Survey.

A two-tailed t-test was applied to estimated regression coefficients. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

[a] The GED credential is given to those who pass the GED test and is intended to signify knowledge of high school subjects.

[b] This outcome is available only for those with any measured employment during year 3. Consequently, the size of the sample (2,217) is less than shown at the bottom of the table, and estimates may not be as reliable as those shown in other rows of the table.

earnings between the second and third years of follow-up was $546, but for those who received a GED during the follow-up period, this earnings growth measure was $797 higher than it would have been had they not earned this credential. This suggests that most of the effect of GED receipt on earnings took until the third year of follow-up to materialize, which is consistent with the theory underlying HCD programs. However, contrary to what one might have expected, GED receipt did not significantly increase average earnings per quarter worked.

As Chapter 2 showed, welfare-to-work program group members were more likely than control group members to earn a GED during the two-year follow-up period. This means that GED receipt and program assignment are correlated, necessitating the inclusion of a separate variable in the analysis to account for other aspects of the welfare-to-work programs. Without such a variable, the effects of case management, sanctions, or other program features might have been attributed to the GED. The coefficients on this program variable (not shown in the table) were almost as large as the coefficients associated with GED receipt. Being in a welfare-to-work program increased year 3 earnings by $796, earnings growth by $447, and earnings per quarter by $221—all statistically significant at the .01 level. This suggests that receipt of a GED certificate was by no means the only way by which program group members benefited from their participation in the welfare-to-work programs.

Having a GED also reduced AFDC benefits in year 3, by $237, or approximately 10 percent of the sample mean. Again, the strongest effect concerned the change in AFDC receipt since the second year of follow-up. The average sample member received AFDC in 1.36 fewer months in year 3 than they had in year 2. Receipt of a GED further reduced this by 0.73 months.

B. Adding Participation Variables

Analyzing the effects of GED receipt, one might ask whether these effects are driven purely by the newly obtained credential or whether they reflect educational gains from participation in adult education programs. Analyses presented in Table 4.3 address this issue by introducing two participation variables into the ordinary least squares regressions whose results were reported in Table 4.2. These variables, obtained from the two-year survey, measure (1) whether sample members ever participated in basic education and (2) how many months they participated. The effects associated with a unit change in each of these variables are presented in Table 4.3, together with the effects of GED receipt.

It is noteworthy that the estimated effects of GED receipt did not change markedly with the addition of the participation variables in Table 4.3. This suggests that the benefits of receiving the GED were mostly unrelated to the education provided in welfare-to-work programs. This does not mean that participation in adult education has no measurable benefits. Instead, it seems that the effects of GED receipt and the effects of participation in adult education were complementary. For purposes of interpretation, this means that the GED effect appeared to represent a "credential" effect, more than a "certification" effect. In other words, rather than certifying successful completion of a course of study, the credential attested to the holder's command of key skills and knowledge. If the reverse had been true, and the GED would primarily have signaled participation in a remedial education program, the estimated GED effect should have been reduced upon inclusion of the participation variables in the regression. Note that this distinction does not address the question of whether the skills measured by the GED are valuable in the la-

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Table 4.3

For Sample Members Without a High School Diploma or GED at Random Assignment: Estimated Effects of Receiving a GED During Years 1-2 on Selected Year 3 Outcomes, Controlling for Participation in Adult Education and Pooled Across 11 Programs

		Estimated Effect of:						
		Receiving a GED[a]		Any Participation in Adult Education		Months Spent in Adult Education		
Outcome	Mean Outcome	Estimate	p-Value	Estimate	p-Value	Estimate	p-Value	R^2
Earnings in year 3 ($)	2,741	+771 ***	0.003	+334	0.114	-25.589	0.257	0.153
Increase in earnings relative to year 2 ($)	546	+598 ***	0.002	+429 ***	0.007	-2.714	0.873	0.039
Earnings per quarter worked in year 3[b] ($)	1,615	+173 *	0.083	-28	0.734	-9.530	0.284	0.170
Total AFDC received in year 3 ($)	2,620	-331 **	0.012	+151	0.156	5.261	0.644	0.299
Number of months receiving AFDC	6.50	-0.47	0.107	+0.83 ***	0.001	-0.016	0.521	0.178
Change in number of months receiving AFDC, compared to year 2	-1.36	-0.53 **	0.017	-0.27	0.128	-0.017	0.376	0.036
Sample size	4,274							

(continued)

Table 4.3 (continued)

SOURCES: MDRC calculations from unemployment insurance (UI) earnings records, AFDC records, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: To compensate for differences in sample members' likelihood of being chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally in the pooled estimates.

Results presented in this table were obtained from ordinary least squares regression analyses, controlling for baseline characteristics, research group (program or control group), and barriers and attitudes measured with the Private Opinion Survey.

A two-tailed t-test was applied to estimated regression coefficients. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

[a] The GED credential is given to those who pass the GED test and is intended to signify knowledge of high school subjects.

[b] This outcome is available only for those with any measured employment during year 3. Consequently, the size of the sample (2,217) is less than shown at the bottom of the table, and estimates may not be as reliable as those shown in other rows of the table.

bor market or whether something else about the credential (for example, its "sheepskin" effect) underlies its apparent payoff. A later section will address this issue.

The participation effects shown in Table 4.3 followed an interesting pattern. Comparing the effects on year 3 earnings and year 3 earnings growth, it appeared that the effects of participation in adult education were even more delayed than the earlier effects on GED receipt were. This makes sense, because participation in adult education programs takes time—more so than pursuit of a GED. As a result, participants may have been diverted temporarily from seeking work, which would have had the effect of diluting early improvements in employment outcomes. The estimated effects of additional months of participation on earnings was generally negative, albeit never statistically significant. It is unclear exactly how these two effects played out, given the participation patterns found in this sample, but the most beneficial course of action seems to have been brief enrollment in an adult education (or GED preparation) program, followed by attainment of a GED. Such a scenario would have been more positive than a much more lengthy spell of adult education without a credential.

C. Separate Effects for Program and Control Group Members

So far, the analyses presented have shown average effects for all sample members, regardless of their research group status. However, it is by no means certain that those assigned to one of the welfare-to-work programs would have experienced the same effects from getting a GED or participating in adult education than those assigned to a control group. In fact, there are two distinct processes that could produce different effects for those in welfare-to-work programs and those excluded from them.

First, we know from Chapter 2 that program group members were more likely than control group members to have earned a GED. In theory, one would expect this to be reflected in the characteristics of those who earned this credential. Presumably, control group members who decided to try to get a GED would have had to be more motivated to make this decision and to follow through on it than program group members who were offered assistance and incentives to do the same. In turn, this difference in underlying motivation should have manifested itself in the employment and welfare outcomes of the two groups of GED recipients. If bias from self-selection accounts for part of the observed GED effect, such bias should be greater among the control group, inasmuch as those GED recipients formed a more "selective" subsample than GED recipients in the program group. Such an effect should be especially strong in a mandatory welfare-to-work environment like the one studied here, in which GED recipients in the control group would have pursued the credential voluntarily, while GED recipients in the program group would have done so under pressure from program rules. Thus, even if selection bias were a relatively minor problem, one should see substantial differences in effects across the research groups.

However, all this assumes that the underlying "true" effects of GED receipt and participation in adult education were the same for both program and control group members, which does not have to be the case. It is possible that program group members benefited more from these services and credentials because they experienced them in the context of a larger program that could help them maximize the benefits of adult education and GED receipt. Program group members had access to postsecondary vocational training, job placement services, case management, and other supports—all of which could enhance the practical effect of a credential on someone's employment outcomes and self-sufficiency.

Table 4.4 supports the second of these two hypotheses. The estimated effects of GED receipt were significantly larger for program group members than they were for controls,[24] suggesting that selection bias was not a serious problem and that a GED credential has stronger effects on employment outcomes when it is attained as part of a larger welfare-to-work intervention offering other supports in addition to GED preparation.

D. Another Way to Capture Possible Selection Bias: Looking for Premature Effects

Although the distribution of GED effects across program and control group members was reassuring, we devised another way to identify the potential effects of uncontrolled selection bias. We did so by estimating effects of the GED on earnings in the first year of follow-up, when most GED recipients did not have the credential for long (if they had earned it at all by then). A positive effect of GED receipt on earnings during that year would have been suspicious, possibly identifying greater motivation, higher inherent skill levels, or other uncontrolled differences benefiting future GED holders.

Carrying out this analysis (not shown in any tables), it appeared that our estimates more than met this test. The estimated effect of GED receipt on total earnings in the first year of follow-up was –$452, which is statistically significant at the .01 level. This suggests that future GED holders were actually working less during the first year of follow-up, possibly because they were engaged in adult education programs instead of working in low-wage jobs. Controlling for participation in adult education (as was done in the analyses presented in Table 4.3) reduced this estimate to –$289, which is still negative and statistically significant at the .10 level. In either case, the estimate was far from being significantly positive—another reassuring pattern of effects that supports our confidence in the longer-term benefits of GED receipt that were presented earlier.

E. Instrumental Variables Estimates

Another popular way to address the issue of selection bias in estimates like the ones presented in this chapter is to use a statistical technique known as *instrumental variables analysis*.[25]

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[24] An F-test was conducted to determine whether the effects for program group members were statistically significantly larger than those found for controls. The p-value for this test (not shown in the table) was 0.0740, indicating a statistically significant difference.

[25] Specifically, instrumental variables estimation addresses bias that occurs when the error term in a regression model is correlated with one or more independent variables in the model. In the case of selection bias, such correlation occurs because an explanatory variable (such as GED receipt) represents an underlying selection process, which is not properly modeled in the regression equation. In the instrumental variables framework, a third variable, the "instrument," is used to predict the explanatory variable and the resulting predicted value of that variable is used in the regression instead. For this procedure to be successful, the instrument must be highly correlated with the explanatory variable but uncorrelated with the outcome variable (except as mediated by the explanatory variable). In our case, we use the randomly created experimental treatment contrast, interacted with program site, as a source of such instruments. To ensure that the analysis does not misattribute program effects to the GED, we included other mediators—such as receipt of job search assistance, participation in basic education, and a proxy variable—to account for the immediate employment effects of the mandate to participate in welfare-to-work programs. For more details and examples of this procedure, see, for example, Angrist, 1990; Angrist and Krueger, 1991; and Heckman and Vytlacil, 1998.

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Table 4.4

For Sample Members Without a High School Diploma or GED at Random Assignment: Estimated Effects of Receiving a GED During Years 1-2 on Selected Year 3 Outcomes, Pooled Across 11 Programs, by Research Status

		Program Group		Control Group		
Outcome	Mean Outcome	Estimated Effect of Receiving a GED[a]	p-Value	Estimated Effect of Receiving a GED[a]	p-Value	R^2
Earnings in year 3 ($)	2,741	+1,151 ***	0.000	+236	0.572	0.153 †
Increase in earnings relative to year 2 ($)	546	+1,246 ***	0.000	-56	0.858	0.038 †††
Earnings per quarter worked in year 3[b] ($)	1,615	+183	0.102	-13	0.940	0.169 n.s.
Total AFDC received in year 3 ($)	2,620	-262 *	0.088	-190	0.368	0.298 n.s.
Number of months receiving AFDC	6.50	-0.12	0.721	-0.15 ***	0.754	0.174 n.s.
Change in number of months receiving AFDC, compared to year 2	-1.36	-1.10 ***	0.000	-0.03	0.936	0.035 ††
Sample size	4,274					

(continued)

Table 4.4 (continued)

SOURCES: MDRC calculations from unemployment insurance (UI) earnings records, AFDC records, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: To compensate for differences in sample members' likelihood of being chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally in the pooled estimates.

Results presented in this table were obtained from ordinary least squares regression analyses, controlling for baseline characteristics, research group (program or control group), and barriers and attitudes measured with the Private Opinion Survey.

A two-tailed t-test was applied to estimated regression coefficients. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

F-tests were conducted to establish whether effects estimated for the program group were statistically significantly different from those estimated for the control group. The results are indicated as: † = 10 percent; †† = 5 percent; ††† = 1 percent; and n.s. = no statistically significant difference.

[a] The GED credential is given to those who pass the GED test and is intended to signify knowledge of high school subjects.

[b] This outcome is available only for those with any measured employment during year 3. Consequently, the size of the sample (2,217) is less than shown at the bottom of the table, and estimates may not be as reliable as those shown in other rows of the table.

For the purpose of this analysis, we did not weight the sample as we did in all previous analyses, because instrumental variables estimates would have become sensitive to the experiences of small groups of sample members in individual sites and subgroups. (Also, we did not know how to judge the statistical properties of weighted instrumental variables estimates.) Table 4.5 summarizes the results.

Looking at the table, the estimates appear to be fairly consistent with those presented in earlier tables. However, Table 4.5 reveals how imprecise the instrumental variables method is, using these data. Estimates that were highly statistically significant in earlier tables now have large p-values, reflecting the large standard errors associated with them. These estimates are so imprecise because the relationship between the instruments used and the explanatory variables of interest is weak. Even though, as Chapter 2 showed, the NEWWS welfare-to-work programs caused significant increases in GED receipt, these effects (and their cross-site variation) were not large enough to make the experimental treatment variables adequate instruments for GED receipt. On the other hand, these estimates did not contradict those presented earlier.[26]

VIII. The Importance of Basic Skills and Their Effects on Employment

In addition to measuring effects of GED attainment, the NEWWS basic education data allow us to measure the effects of changes in basic skill levels, the second set of mediators presented in the introduction to this chapter. As discussed earlier, data on follow-up reading skills were collected at the time of the two-year follow-up interview, using the TALS test to measure literacy. Not all survey respondents in all sites were administered these tests. The TALS data were available only for those assigned to HCD programs in Atlanta, Grand Rapids, and Riverside (and not in possession of a high school diploma or GED at baseline). The CASAS math scores were collected only for a subsample of this "TALS sample," namely, those whose youngest child was older than 6 at baseline.[27]

A. Comparing GED Effects Across the Samples

Because it is possible that average effects of GED receipt would be different in these two subsamples, we repeated the basic analyses presented in earlier tables for these samples. Table 4.6 shows effects of GED receipt on the basic year 3 earnings and AFDC receipt measures for the full sample (the same as in Table 4.2), the TALS sample, and the CASAS sample. At $1,238 and $1,508, respectively, the earnings effects associated with GED receipt were larger in the TALS and CASAS samples than in the full sample (where we found an effect of $836). This may reflect the different composition of the TALS and CASAS samples (most notably, the fact that only three of the eleven NEWWS programs were represented), but it also underscores the sensitivity of the GED effects to sample selection and model specification.

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[26] An interesting aspect of these analyses, not presented in Table 4.5, is the fact that these instrumental variables analyses uncovered effects of other mediating variables in the relationship between welfare-to-work programs and employment outcomes. The only mediator that was consistently statistically significant was participation in job search, which, for example, was associated with an increase of $1,834 in year 3 earnings—statistically significant at the 5 percent level.

[27] This distinction made it easier to administer a special survey module for parents of younger children. There is no substantive reason to exclude parents of younger children from the math test.

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Table 4.5

For Sample Members Without a High School Diploma or GED at Random Assignment: Estimated Effects of Receiving a GED During Years 1-2 on Selected Year 3 Outcomes, Pooled Across 11 Programs, Controlling for Participation in Adult Education and Using Unweighted Instrumental Variables Estimators

		Estimated Effect of:					
		Receiving a GED[a]		Any Participation in Adult Education		Months Spent in Adult Education	
Outcome	Mean Outcome	Estimate	p-Value	Estimate	p-Value	Estimate	p-Value
Earnings in year 3 ($)	2,741	+394.74	0.920	+6.10	0.872	-40.20	0.925
Increase in earnings relative to year 2 ($)	546	+2,818.41	0.302	+4.53	0.863	-76.27	0.796
Earnings per quarter worked in year 3[b] ($)	1,615	+957.05	0.408	-0.50	0.968	-17.47	0.896
Total AFDC received in year 3 ($)	2,620	-705.41	0.830	+11.67	0.712	-128.93	0.717
Number of months receiving AFDC	6.50	-3.14	0.663	+0.04	0.575	-0.38	0.626
Change in number of months receiving AFDC, compared to year 2	-1.36	-7.66 **	0.045	+0.04	0.222	-0.41	0.322
Sample size	4,274						

(continued)

Table 4.5 (continued)

SOURCES: MDRC calculations from unemployment insurance (UI) earnings records, AFDC records, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: Results presented in this table were obtained using instrumental variables analyses. Such analyses attempt to control for unmeasured differences between those who received a GED and those who did not (which might bias estimates from ordinary least squares analyses). Instruments were derived from the experimental (program-control group) contrast, created as part of the overall NEWWS Evaluation. Instrumental variables estimates tend to have large standard errors and are sensitive to statistical manipulations such as weighting. Hence, no weights were used in this analysis.

A two-tailed t-test was applied to estimated regression coefficients. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

[a] The GED credential is given to those who pass the GED test and is intended to signify knowledge of high school subjects.

[b] This outcome is available only for those with any measured employment during year 3. Consequently, the size of the sample (2,217) is less than shown at the bottom of the table, and estimates may not be as reliable as those shown in other rows of the table.

Table 4.6

For Sample Members Without a High School Diploma or GED at Random Assignment: Estimated Effects of Receiving a GED During Years 1-2 on Selected Year 3 Outcomes, by Availability of TALS Document Literacy and CASAS Math Test Scores

Outcome	Mean Outcome	Estimated Effect of Receiving a GED[a]	p-Value	R^2
Full Sample[b]				
Earnings in year 3 ($)	2,741	+836 ***	0.001	0.152
Total AFDC received in year 3 ($)	2,620	-237 *	0.059	0.298
Sample size	4,274			
TALS Sample[c]				
Earnings in year 3 ($)	2,652	+1,238 ***	0.002	0.206
Total AFDC received in year 3 ($)	3,018	-295	0.157	0.299
Sample size	2,296			
CASAS Sample[d]				
Earnings in year 3 ($)	2,894	+1,508 ***	0.009	0.179
Total AFDC received in year 3 ($)	2,501	-161	0.549	0.279
Sample size	1,323			

SOURCES: MDRC calculations from unemployment insurance (UI) earnings records, AFDC records, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: To compensate for differences in sample members' likelihood of being chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally in the pooled estimates.

Results presented in this table were obtained from ordinary least squares regression analyses, controlling for baseline characteristics, research group (program or control group), and barriers and attitudes measured with the Private Opinion Survey.

A two-tailed t-test was applied to estimated regression coefficients. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

[a] The GED credential is given to those who pass the GED test and is intended to signify knowledge of high school subjects.

[b] The full sample includes all sample members in 11 NEWWS programs who did not have a high school diploma or GED certificate at random assignment and who responded to the Two-Year Client Survey. This is the same sample used in Tables 4.1 through 4.5.

[c] The TALS sample includes all sample members in Atlanta, Grand Rapids, and Riverside who (1) were assigned to an education-focused program or to the control group, (2) did not have a high school diploma or GED certificate at random assignment, and (3) responded to the Two-Year Client Survey and completed the TALS test.

[d] The CASAS sample includes all sample members in the TALS sample who also completed a CASAS math test. For logistic reasons, this test was not administered to sample members who had children between 3 and 6 years old.

B. Effects of Variation in Literacy Skills

In this section we present effects of basic literacy on employment, earnings, and welfare outcomes. There are three ways to look at these effects. First, we present these effects without controlling for educational attainment (GED status), which incorporates into the analysis the differences in skill levels between GED holders and non-GED holders. Then we introduce the GED into the analysis, which means that the estimates now capture the importance of basic literacy *over and above* the effect of having a GED. In a third version of the analysis, we add baseline test scores to the equation. This effectively limits the variation in literacy to measured literacy gains between baseline and follow-up. In theory this should not change the effects by much, but in practice the value of "preexisting" literacy skills may differ from that of newly acquired skills.

Table 4.7 presents these three perspectives on the effects of literacy skills.[28] The top panel shows the effects of changes in TALS scores, without controlling for GED receipt or baseline test scores. The analyses found that if TALS scores were higher by a standard deviation, year 3 earnings would increase by $355 (or 13.4 percent of mean earnings), and earnings per quarter worked would be higher by $127 (8.0 percent of mean earnings per quarter). There were no comparable effects on earnings growth (since year 2) or AFDC receipt. The lack of a statistically significant effect on earnings growth may be explained by the fact that changes in basic skills are not immediate, as GED receipt is, which makes it unlikely that one would see a sudden change in the earnings or earnings trajectory of those who increase their skills through adult education.

The second panel shows that controlling for GED status makes no significant difference in the size of most coefficients associated with literacy and math skills. All this reaffirms that the GED, as measured in this study, has primarily a "credential" effect, not representing fundamentally different skill levels but having an independent effect regardless of those underlying skills. This is consistent with other studies[29] that found that GED receipt was not accompanied by increased reading and math skills. All these findings, including those presented in Chapter 3 of this report, suggest that welfare recipients who receive a GED generally are close to being able to pass the GED test before they enter the programs preparing them. Additional skills taught in GED preparation classes apparently are not identified by basic skills tests such as TALS and CASAS.

The third panel controls for baseline test scores, in addition to controlling for GED receipt. In theory, this should not change the effects associated with variation in follow-up test scores, provided that skills acquired between baseline and follow-up are equivalent to skills acquired earlier. The only difference is that the variation in skills used in the analysis is more limited, making the analysis less precise. However, the table seems to show otherwise, showing generally smaller (and nonsignificant) coefficients associated with basic skills acquired after random assignment. One difficulty in interpreting this finding is that two-year follow-up test scores often were lower than initial test scores, possibly reflecting differences in the administration of the

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[28] Note that all three analyses include the participation variables introduced in Table 4.3. Thus, apparent effects from literacy gains and GED receipt are not confounded with the education that produced them.

[29] Similar patterns were found by Martinson and Friedlander, 1994; Friedlander et al., 1994; and Quint et al., 1994.

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Table 4.7

For Sample Members Without a High School Diploma or GED at Random Assignment: Estimated Effects of Receiving a GED During Years 1-2 and of TALS Document Literacy Test Scores on Year 3 Outcomes, Pooled Across Three Education-Focused Programs

Outcome	Mean Outcome	Estimated Effect of Receiving a GED[a] Effect	Estimated Effect of Receiving a GED[a] p-Value	Estimated Effect of a Standard Deviation Change in the TALS Literacy Score Effect	Estimated Effect of a Standard Deviation Change in the TALS Literacy Score p-Value	R^2
Not controlling for GED status						
Earnings in year 3 ($)	2,652	N/a		+355 ***	0.001	0.207
Increase in earnings relative to year 2 ($)	548	N/a		+47	0.573	0.039
Earnings per quarter worked in year 3[b] ($)	1,582	N/a		+129 **	0.012	0.227
Total AFDC received in year 3 ($)	3,018	N/a		+8	0.893	0.298
Number of months receiving AFDC	7.28	N/a		-0.01	0.916	0.177
Change in number of months receiving AFDC, compared to year 2	-1.36	N/a		-0.00	0.968	0.030
Controlling for GED status						
Earnings in year 3 ($)	2,652	+1,087 ***	0.006	+316 ***	0.004	0.209
Increase in earnings relative to year 2 ($)	548	+472	0.127	+30	0.719	0.040
Earnings per quarter worked in year 3[b] ($)	1,582	+275 *	0.084	+117 **	0.023	0.230
Total AFDC received in year 3 ($)	3,018	-304	0.148	+19	0.747	0.299
Number of months receiving AFDC	7.28	-0.60	0.166	+0.01	0.939	0.178
Change in number of months receiving AFDC, compared to year 2	-1.36	-0.94 ***	0.008	+0.03	0.756	0.033

(continued)

Table 4.7 (continued)

Outcome	Mean Outcome	Estimated Effect of Receiving a GED[a]		Estimated Effect of a Standard Deviation Change in the TALS Literacy Score		R^2
		Effect	p-Value	Effect	p-Value	
Controlling for GED status and baseline TALS score[c]						
Earnings in year 3 ($)	2,665	+996 **	0.015	+108	0.419	0.212
Increase in earnings relative to year 2 ($)	572	+460	0.146	-17	0.869	0.041
Earnings per quarter worked in year 3[b] ($)	1,589	+255	0.120	+79	0.213	0.229
Total AFDC received in year 3 ($)	3,006	-270	0.203	+64	0.357	0.298
Number of months receiving AFDC	7.31	-0.56	0.207	+0.16	0.270	0.179
Change in number of months receiving AFDC, compared to year 2	-1.36	-1.06 ***	0.003	+0.02	0.850	0.034
Sample size	2,296					

SOURCES: MDRC calculations from unemployment insurance (UI) earnings records, AFDC records, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: To compensate for differences in sample members' likelihood of being chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen. In addition, each site was weighted equally in the pooled estimates.

Results presented in this table were obtained from ordinary least squares regression analyses, controlling for baseline characteristics, research group (program or control group), and barriers and attitudes measured with the Private Opinion Survey.

A two-tailed t-test was applied to estimated regression coefficients. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

[a] The GED credential is given to those who pass the GED test and is intended to signify knowledge of high school subjects.

[b] This outcome is available only for those with any measured employment during year 3. Consequently, the size of the sample (2,217) is less than shown at the bottom of the table, and estimates may not be as reliable as those shown in other rows of the table.

[c] Inclusion of baseline test scores essentially limits the effects of variation in the TALS scores to variation that originated between random assignment and the two-year follow-up interview. This variation is less extensive than the overall variation in TALS scores, which means that estimates are less precise.

tests.[30] However, it is quite possible that recent test score gains were not immediately reflected in sample members' earnings and welfare outcomes.

Last, we repeated the analyses presented in Table 4.7, adding CASAS math scores to the equation (not shown in tables). Doing so did not substantially change the estimated effects shown in Table 4.7. The coefficient on the CASAS variable itself was almost never statistically significant.

IX. Conclusions

This chapter examined how key educational outcomes of welfare-to-work programs relate to subsequent employment and welfare outcomes. We found some evidence that GED receipt is associated with higher earnings and lower welfare receipt, although we were unable to confirm these findings using more advanced statistical methods—mostly because NEWWS program effects on GED receipt were too small. On the other hand, none of our analyses suggested that the personal characteristics and motivation of GED recipients were responsible for the apparent effects of the credential.

Our findings do not suggest that the effects of the GED represent either underlying effects of participation in basic education or underlying differences in basic skills. Both participation in basic education and increased basic skills have independent positive effects on earnings and earnings growth, although participation in basic education is also associated with longer welfare spells.

All this suggests that receipt of education credentials and increases in basic skills are good indicators with which to assess the success of basic education programs for welfare recipients. A focus on improving these mediators should result in better long-term outcomes for welfare recipients. However, future research should examine the effects of GED receipt in the context of welfare-to-work programs that have greater effects on this mediator.

Finally, this chapter contributes some new lessons to the ongoing debate about the value of basic education and GED receipt for welfare recipients and other low-income populations. Our analyses suggest that welfare recipients do benefit from receiving a GED, supporting findings by Quint et al. (1997) and others. However, we were unable to reproduce our findings using instrumental variables estimation, mostly because our estimates lacked precision.

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[30] Follow-up tests were administered in sample members' homes as part of a larger interview, whereas baseline tests were administered in the welfare-to-work program office, where there were fewer distractions and perhaps greater pressure to do well on the test.

Chapter 5

A Comparison of Program Options for Welfare Recipients in Need of Basic Education

I. Introduction

The NEWWS Evaluation features 11 welfare-to-work programs in seven sites across the United States. Each program is being evaluated using a random-assignment research design, allowing us to reliably estimate the effects of the various programs on the people assigned to participate in them. However, besides learning about the effectiveness of each program, the fact that there are 11 different programs makes it possible to learn more. By systematically comparing program effects across all programs and sites, we can learn which approach may be most effective with certain groups of welfare recipients.

The purpose of this chapter is to describe and analyze the variation in program effects found in the NEWWS Evaluation, focusing on a subsample of the welfare recipients served by the NEWWS programs, namely, those lacking a high school diploma or GED.[1] It is hoped that such analysis will lead to greater understanding of which approaches are most successful with this population.

Specifically, the analyses presented here examine variation in program effects along a range of program dimensions first introduced by Freedman et al.[2] In their analysis of two-year program effects for the full NEWWS sample (including those who did have a high school diploma or GED), Freedman et al. developed a typology of NEWWS programs, dividing them first by their level of enforcement and then by their substantive focus, which was considered either "employment-focused" or "education-focused." In addition, Freedman et al. paid attention to each site's sequencing of program activities and case management structure. The 11 programs featured in the NEWWS Evaluation were characterized along the dimensions shown in Figure 5.1 (adapted from Freedman et al., 1999).

Inasmuch as Freedman et al. presented effects for all these programs and separately discussed the effects for participants without a high school diploma or GED, why are we revisiting these comparisons in this chapter? We do so for several reasons.

First, Freedman et al. had only two years of follow-up data when they prepared their report. Although this is a long enough follow-up period to present general program effects, it may be too short to find effects for those lacking a high school diploma or GED. Especially when these sample members participate in basic education programs, it may take longer for positive impacts to materialize for them. At present, we have three years of follow-up data, allowing us to tell a more complete story about this subgroup of welfare recipients.

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[1] About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math and reading portion of the appraisal test or had limited English and, thus, were determined to be in need of basic education.

[2] Freedman et al., 1999.

Figure 5.1

Typology of NEWWS Programs

Employment-Focused Approach		**Education-Focused Approach**	
Job Search First	Varied First Activity	Education or Training First	
High Enforcement	**High Enforcement**	**High Enforcement**	**Low Enforcement**
Atlanta LFA	*Portland*	Atlanta HCD	Detroit
Grand Rapids LFA		Grand Rapid HCD	*Oklahoma City*
Riverside LFA		Riverside HCD	
		Columbus Integrated	
		Columbus Traditional	

NOTES: LFA = Labor Force Attachment; HCD = Human Capital Development. Programs in *italics* had integrated case management.

Second, this chapter expands on the work of Freedman et al. and others in that it uses more advanced statistical techniques to sort out different causes of variation in impacts across different welfare-to-work programs.[3] While the dimensions shown in Figure 5.1 are our primary focus, differences in impacts across sites and programs can be attributed to many other dimensions, including variation in local environments, variation in sample characteristics, and aspects of program operations other than the ones highlighted. In many cases, what accounts for cross-site variation in program effects is not obvious, but this chapter will make an effort to find out.

In addition to covering 11 programs in seven sites, the NEWWS Evaluation has a feature that not many evaluations of social welfare programs have: random assignment to multiple programs in a single site. In Atlanta, Grand Rapids, Riverside, and Columbus, welfare recipients were assigned not just to a single NEWWS program but to one of two different programs (or to a control group). In the first three sites, the assignment was either to a Human Capital Development (HCD) program or to a Labor Force Attachment (LFA) program.[4] In Columbus, assignment was to a Traditional program or to an Integrated program featuring integrated case management. Because of random assignment, we can estimate fully experimental effects of assignment to one of these programs versus another, holding constant all other aspects of the program environment and sample composition. For analyses that go beyond this fully experimental framework, we instead have to rely on nonexperimental methods to separate programmatic differences from other factors that might interact with the effects of NEWWS programs. The large samples available in the NEWWS Evaluation make this study a good research environment in which to undertake such analyses.

The structure of this chapter is straightforward. Section II presents key findings, and section III introduces the methods used in the analysis; section IV describes the sample and the program environment. Section V then presents impacts for all 11 programs, section VI discusses differences in impacts across the programs, and section VII presents effects for a number of subgroups defined across the sample. Section VIII then offers conclusions. The purpose of this chapter is not to tell a comprehensive story about all relevant program effects for sample members entering without a high school diploma or GED. Instead, we focus on a few outcomes, attempting to identify program characteristics that best explain variation in effects concerning these outcomes.

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[3] For a discussion of the importance of understanding variation in effects across different program approaches see, for example, Greenberg and Wiseman, 1992; and Greenberg et al., 1993. For an application of some of the approaches used in this chapter, see Bloom et al., 1993, 1997.

[4] See Chapter 2 for an overview of the two types of programs.

II. Summary of the Findings

- **Generally, the welfare-to-work programs studied were successful in increasing sample members' earnings and reducing their reliance on welfare.**

On average, welfare recipients who entered the 11 programs in the NEWWS Evaluation without a high school diploma or GED increased their three-year earnings by $1,212, or 21.7 percent, and received $1,056 fewer welfare benefits (a reduction of 10.6 percent). Such favorable impacts were found for most of the individual programs and subgroups we studied.

- **Some significant differences in program effects across the 11 programs are attributable to differences in characteristics of the welfare recipients served by these programs.**

Although patterns of impacts across the 11 welfare-to-work programs were largely unaffected by our efforts to control for differences in individual characteristics across the sites, some programs—most notably those in Atlanta and Detroit—had more modest effects than the other programs, possibly because they served welfare recipients for whom such effects were more difficult to achieve.

- **Experimental analyses show that education-focused programs may have smaller impacts than employment-focused programs, at least during the first two years of follow-up. Also, programs featuring integrated case management may have stronger impacts than programs with a traditional separation of income maintenance and welfare-to-work tasks.**

The strongest comparisons featured in this chapter involve sites in which dual random assignment created two separate program groups, each with its own program approach. In three sites—Atlanta, Grand Rapids, and Riverside—we studied education-focused and employment-focused programs side by side. Although impact estimates were stronger for the employment focused programs in all three sites, the differences were statistically significant only in Grand Rapids. In that site, three-year program effects on earnings were more than twice as large for the employment-focused LFA program than for the education-focused HCD program, although this difference was no longer statistically significant in the third year. Statistically significant differences between program options were also found in Columbus, where the Integrated program had significantly greater earnings and AFDC impacts than the Traditional program.

- **Other comparisons found that programs with strong enforcement had greater effects on welfare receipt, but not on earnings. Also, Portland's**

flexible approach regarding participants' first activity was more successful than other employment-focused approaches.

After controlling for variation in individual characteristics across the sites, high enforcement programs remained more successful in terms of reducing AFDC receipt. For example, high enforcement education-focused programs reduced three-year AFDC receipt by $1,170, compared to $160 for similar programs without strong enforcement. Also, the program in Portland was consistently stronger than the other employment-focused programs (in Atlanta, Grand Rapids, and Riverside), even after controlling for differences in the populations served by these programs. For example, the Portland program increased three-year earnings by $2,433, compared to $1,698 for the other three programs combined.

- **Tentative evidence from cross-site comparisons suggests that grant levels and unemployment rates are important site-level predictors of program effectiveness.**

We attempted to use variation in economic circumstances and the larger program environments in our data to identify predictors of program success other than those captured by narrow program characteristics. Doing so resulted in some evidence that higher grant levels were associated with greater program effects and that higher unemployment rates were associated with smaller program effects, especially on earnings.

- **Subgroup analyses identified initial reading levels and personal barriers as important predictors of program success. Lower reading levels led to smaller program effects on earnings. Also, personal problems and other barriers significantly reduced program effectiveness, except in cases where these barriers were directly related to participants' concerns about leaving their children to go to work or school. It is possible that program services may have mitigated the negative effects of the latter concerns.**

In our subgroup analyses, we focused on a narrow set of subgroup dimensions that were introduced in Chapter 2. Using only those, we identified low reading scores and self-reported personal or family problems as significant hurdles to favorable program effects. Sample members who, on the baseline Private Opinion Survey, indicated that they had personal or family problems but who did *not* have many concerns about leaving their children to go to work or school were the only subgroup studied that did not experience positive program effects on earnings.

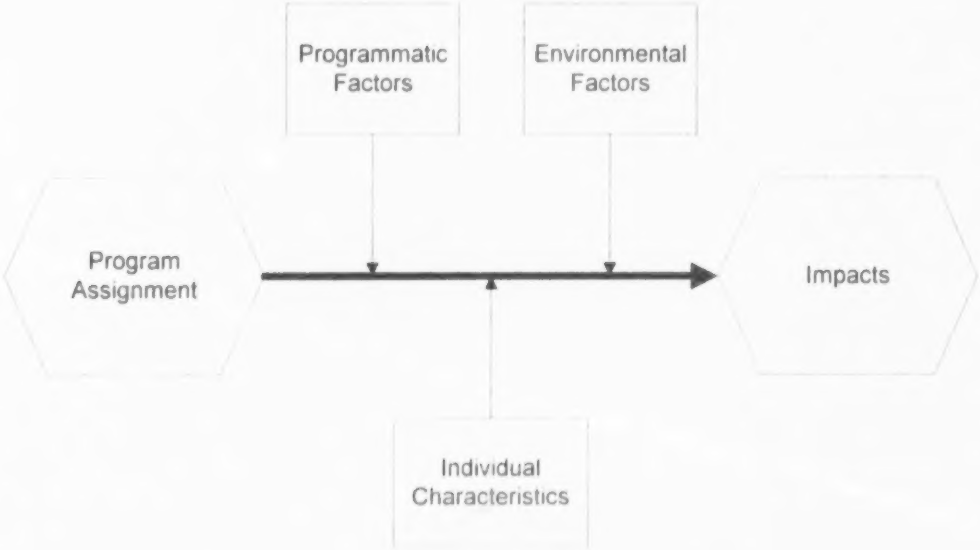
III. Methods

Figure 5.2 describes the relationship between assignment to a specific NEWWS program and subsequent program impacts. The NEWWS Evaluation is principally designed to answer questions about the thick horizontal arrow shown in the figure—that is, about the relationship between program assignment and program impacts. Understanding this relationship means that we know the effect of a specific program.

However, researchers and policymakers often are interested in the other concepts illustrated in Figure 5.2. For example, program developers may want to know how different programmatic factors contributed to the overall relationship between program assignment and program effects. In other words, they want to answer questions about the specific contributions of such program dimensions as the focus (education or employment), the level of enforcement (high or low) or the structure of case management (traditional or integrated). Others may want to learn more about how the program effects vary for different subgroups of participants; such issues are captured by the box at the bottom left of the figure. Finally, a program is subject to environmental factors such as employment opportunities, welfare rules, grant levels, and social and cultural norms. Again, such factors may be significant to policymakers and program developers; a program that succeeds in a strong labor market may be ineffective during a recession, and a program that operates one way in a small-town setting like Grand Rapids may perform differently in a metropolis like Atlanta.

Figure 5.2

Relationship Between Program Assignment and Impacts



The primary analytical problem confronted in this chapter is the fact that variation in these different sets of moderating factors is often difficult to isolate, especially when the number of sites in a study is limited, as is the case here. Thus, it may seem that a site with a strong enforcement policy is more successful than a site lacking such a policy, but at the same time the former site may have a stronger economy, lower grant levels, a different welfare population, or a different program focus. How do we separate all these factors?

Our analyses distinguish among multiple factors in three ways. First, we focus on those sites where sample members were randomly assigned to different programs. Such random assignment designs produce the best estimates of the effects of variation in program characteristics. Relying on these experimental comparisons, we can draw conclusions about the relative effectiveness of HCD programs compared with LFA programs. We can also measure the relative benefit of using integrated rather than traditional case management (in which welfare-to-work activities and income maintenance activities are carried out by separate caseworkers).

Second, moving away from randomly created program differences, we approach other dimensions—such as enforcement, program focus, and sequencing—by comparing effects across all 11 programs, holding constant differences in the individual characteristics of the people served by each program. Statistically, these analyses are conducted as follows:

1. The first step is to estimate a pooled regression model, which identifies the overall effect on a particular outcome of assignment to any NEWWS program.

2. The single NEWWS program variable is replaced by 11 program variables, each identifying a single program. A simple F-test is conducted to ascertain whether this change in the regression model significantly improves the fit of the regression.[5] If it does, the program effects are known to be statistically significantly different from one another.

3. The next step is to interact the original NEWWS program variable with a set of individual characteristics of sample members in the study. Adding these interactions to the regression model identifies the extent to which program effects are themselves dependent on the characteristics of those being served. To the extent that those characteristics vary across the programs being compared, this variation might be falsely attributed to programmatic characteristics, thereby inflating apparent differences in program effects across the 11 NEWWS programs.

4. After accounting for variation in individual characteristics, the 11 program effects will be less different from one another, and the F-test used to assess the extent of variation across them may no longer find statistically significant variation in program effects.

5. All these results are reported, identifying (a) the degree to which NEWWS programs vary in their effectiveness and (b) the extent to which such variation results from either programmatic differences or individual differences.

Third, the analysis will attempt to incorporate variation in the labor market and welfare environment across the sites. This is done by taking the estimated program effects from the previous step (already adjusted for variation in individual characteristics across the sites) and regressing these estimates on site-level variables capturing unemployment, job growth, welfare grant levels, etc. Such a regression has few degrees of freedom, which means that results will be tentative, but they should be informative nonetheless.

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[5] This is a type of Chow test, measuring the statistical significance of imposing a restriction on a set of coefficients. In this context, the unrestricted model is the model in which program effects are allowed to vary across the 11 programs. Replacing 11 program variables with a single NEWWS variable forces the program effect to be the same for all 11 programs. This restriction reduces the fit (and explanatory power) of the regression model. The extent of the reduction in explanatory power is a measure of the statistical significance of the variation among individual program effects. See Kennedy, 1992, p. 57, for details.

Finally, we end the chapter by exploring how program effects vary across a number of different subgroups pooled across the 11 programs. Again, we assess the statistical significance of differences in impact estimates across the subgroups.

IV. Sample and Environment

Table 5.1 describes the sample for the analyses presented in this chapter, including characteristics for each sample at the seven NEWWS sites.[6] Although not shown in the table, all differences across the sites were statistically significant, and yet the absolute magnitude of the differences often was small. Here is how the sites compared:

- On average, sample members in *Atlanta* were older, had more and older children, and were far more likely than sample members in other sites to be living in public housing. They also were least likely to be enrolled already in an education or training activity when they were randomly assigned to the Positive Employment and Community Help (PEACH) program (Georgia's version of the JOBS program).

- Sample members in *Grand Rapids* were most likely to be enrolled in education and training activities, both at the time of random assignment and in the preceding year, when 45.0 percent were enrolled in such an activity. These sample members also were relatively young (26.8 years old on average), and many had young children. Almost 50 percent of sample members in Grand Rapids had a child age 2 or less at the time of random assignment.

- Most Hispanic sample members in the study lived in *Riverside*. Also, in this site, fewer sample members were never married (about two-thirds were married, separated, divorced, or widowed). Sample members in Riverside also were older, had older children, and had more recent work experience than sample members in most other sites. The percentage of sample members who, as a child, were in a household receiving AFDC was lower than in any of the other sites.

- Sample members in *Columbus* were least likely to be employed at random assignment or to have ever worked full time for six months or more for one employer. They also had long stays on welfare, and they were least likely to have received education or training in the year preceding random assignment.

- In *Detroit*, more sample members had never been married than in any of the other sites. Welfare stays in Detroit were long, and almost half of all sample members had been in a household receiving AFDC when they grew up. Fewest sample members in Detroit had any earnings in the year preceding random

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[6] For more details on the samples and data sources used throughout this report, please refer to Appendix A.

Table 5.1

For Sample Members Without a High School Diploma or GED at Random Assignment: Selected Characteristics of Sample Members, by Site

Characteristic	Atlanta	Grand Rapids	Riverside	Columbus	Detroit	Oklahoma City	Portland
Demographic Characteristics							
Gender (%)							
Male	3.6	4.1	9.9	6.4	3.6	5.2	5.7
Female	96.4	95.9	90.1	93.6	96.4	94.8	94.3
Age (%)							
Less than 19	0.0	10.7	0.8	0.3	4.8	19.9	0.0
19-24	10.1	36.9	15.8	13.0	28.9	31.9	24.6
25-34	52.9	36.8	50.0	57.0	40.8	34.3	56.7
35-44	29.9	12.4	25.9	24.5	19.7	11.6	16.8
45 and over	7.2	3.3	7.5	5.1	5.8	2.3	2.0
Average age (years)	33.1	26.8	32.1	31.8	29.5	25.8	29.3
Ethnicity (%)							
Black	93.0	39.0	16.3	44.4	85.3	21.6	19.8
White	4.8	47.0	39.2	53.5	12.5	64.3	68.6
Hispanic	1.2	11.8	39.4	0.5	0.7	6.0	5.1
Other	0.9	1.1	3.9	1.6	1.1	0.8	3.3
Family Status							
Marital status (%)							
Never married	62.1	63.4	34.5	49.6	69.7	41.2	52.1
Married, living with spouse	1.6	3.6	9.1	9.4	3.1	4.4	1.6
Separated	21.0	18.4	32.7	22.7	14.9	35.1	22.2
Divorced	13.7	13.6	21.8	17.3	10.8	18.4	23.5
Widowed	1.7	1.0	2.0	1.0	1.5	0.9	0.7
Number of children (%)							
1	32.1	45.4	35.6	35.0	38.4	54.1	32.9
2	30.6	35.2	30.8	31.9	28.9	27.9	33.2
3 or more	37.4	19.4	33.6	33.1	32.7	18.0	33.9
Age of youngest child (%)							
2 or under	0.4	49.9	7.0	1.4	41.0	51.3	43.5
3 to 5	38.6	20.5	49.9	43.9	24.0	20.2	27.8
6 or over	61.0	29.5	43.2	54.7	35.0	28.4	28.7
Had a child as a teenager (%)	49.6	61.4	36.7	45.2	55.5	63.5	42.8

(continued)

Table 5.1 (continued)

Characteristic	Atlanta	Grand Rapids	Riverside	Columbus	Detroit	Oklahoma City	Portland
Labor Force Status							
Worked full time for 6 months or more for one employer (%)	61.9	49.8	62.9	32.3	34.3	52.0	66.5
Any earnings in past 12 months (%)	21.0	37.7	34.0	19.5	15.1	59.1	31.3
Currently employed (%)	5.1	8.6	7.5	2.8	5.1	6.6	7.2
Education Status							
Highest grade completed in school (average)	10.0	10.0	10.1	10.0	10.2	9.9	10.0
Enrolled in education or training in past 12 months (%)	10.2	45.0	15.5	8.9	20.2	23.5	15.2
Currently enrolled in education or training (%)	5.8	36.8	9.7	8.1	28.6	12.0	8.4
Public Assistance Status							
Total prior AFDC receipt (%)[a]							
None	0.4	0.1	0.9	6.9	2.9	47.1	1.1
Less than 1 year	15.9	20.0	29.9	6.1	12.3	18.3	16.9
1 year or more but less than 2 years	6.8	18.1	10.2	6.3	7.8	11.0	13.9
2 years or more but less than 5 years	21.0	30.2	26.3	25.2	21.3	14.0	32.5
5 years or more but less than 10 years	22.3	16.7	17.3	26.3	23.6	6.8	24.4
10 years or more	33.6	15.0	15.5	29.2	32.0	2.7	11.2
Raised as a child in a household receiving AFDC (%)	32.2	41.8	21.9	34.0	48.1	28.8	33.2
Housing Status							
Current housing status (%)							
Public housing	42.2	2.8	3.0	15.2	5.7	5.1	8.8
Subsidized housing	21.8	10.2	7.7	23.9	1.4	5.5	15.2
Emergency or temporary housing	1.2	2.2	1.1	1.6	0.8	16.1	3.1
None of the above	34.8	84.9	88.1	59.3	92.1	73.3	72.8
Sample size	2,248	1,862	4,695	3,073	1,925	2,585	1,861

(continued)

Table 5.1 (continued)

SOURCE: MDRC calculations from information routinely collected by welfare staff.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Each site was weighted equally in the pooled estimates.

Distributions may not add up to 100 percent because of rounding.

All differences across the sites were statistically significant.

[a] This refers to the total number of months accumulated from at least one spell on an individual's own or spouse's AFDC case. It does not include AFDC receipt under a parent's name.

assignment. On the other hand, 28.6 percent already were enrolled in education or training at random assignment—second only to Grand Rapids, the other site in Michigan.

- *Oklahoma City* had the youngest sample of the seven sites. More than half the sample in this site were under 25, and 63.5 percent had a child as a teenager. However, in other ways members of the Oklahoma sample were less disadvantaged than participants in the other sites: 59.1 percent had worked in the previous year, and more than half had been on welfare for less than a year.

- The ethnic composition of the sample in *Portland* was similar to that in Oklahoma, with a majority of sample members who were white. Sample members in Portland also were most likely to have worked full time for six months for one employer.

Thus, looking across these sites, there is a great deal of variation, both in sample members' work-readiness and in their current and recent participation in education and training. One might expect all this to be reflected in the program effects found in these sites, although the expected pattern of effects is not obvious. For example, in sites like Atlanta, Riverside, and Portland few sample members had recent experience with education and training. In those sites, one might expect the programs to make a bigger difference in that regard than in sites such as Grand Rapids and Detroit, where many sample members already participated on their own. Another important factor might be the long-term welfare dependency of sample members across the sites. On the one side, we find Oklahoma City, where only about 9.5 percent of the sample had received AFDC for five years or more. On the other side, we find sites like Columbus, Detroit, and Atlanta, in which around 55 percent received AFDC that long. One might expect program effects to be larger in the latter group of sites, where control group members would be less likely to leave AFDC on their own. On the other hand, however, samples of long-term recipients tend to be more disadvantaged overall, making it more difficult to serve them. In any case, it seems important to consider variation in sample characteristics as we attempt to explain variation in program effects across the sites. In later sections, we will use statistical controls to do so, and we also will conduct subgroup analyses to uncover how some of these characteristics interact with the programs across the different sites.

Table 5.2 describes the sites in a different way, using aggregate statistics covering the economic and welfare environment. Although these statistics do not always cover the same calendar years, they give a useful overview of the environment. Looking at the table, it appears that, in 1997, unemployment was below 5 percent in all sites except Riverside. Between 1996 and 1997, unemployment fell in all the sites except Oklahoma City, and this reduction was strongest in Grand Rapids. Employment growth, measured between 1990 and 1996, was strongest in Atlanta, Grand Rapids, and Portland, but it was generally strong in all the sites.

Poverty rates, estimated by the Census Bureau for 1995 only, varied substantially across the sites. Highest in Atlanta and Detroit, they ranged from highs of 20.9 and 20.6 percent in those two sites to a low of 9.6 percent in Grand Rapids. This means that sample members in the latter site (and in comparable places such as Portland and Columbus) live in communities that may of-

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Table 5.2

Selected Characteristics of the Local Environment

Characteristic	Atlanta	Grand Rapids	Riverside	Columbus	Detroit	Oklahoma City	Portland
Employment Statistics[a]							
Local unemployment rate (%)							
1996	3.8	5.6	7.7	3.1	4.5	4.1	4.5
1997	3.7	4.5	6.9	2.9	3.9	4.1	4.3
Change between 1996 and 1997	-0.1	-1.1	-0.8	-0.2	-0.6	+0.0	-0.2
Change in number of jobs between 1990 and 1996 (%)	+19.6	+16.3	+8.6	+9.4	+7.8	+5.9	+18.8
Poverty Rate and Income[b]							
1995 county poverty rate (%)	20.9	9.6	14.3	11.7	20.6	17.1	13.5
1995 county median household income ($)	35,932	39,240	36,189	37,221	32,382	31,221	34,966
Welfare Statistics[c]							
Change in caseload 1995-1998[d] (%)	-46.1	-39.4	-21.9	-40.9	-39.4	-47.8	-54.8
Maximum monthly AFDC grant in 1996 ($)	280	459	607	341	459	307	460
Maximum AFDC grant, as % of median income/12	9.4	14.0	20.1	11.0	17.0	11.8	15.8

SOURCES: MDRC calculations using data from the Census Bureau, the Bureau of Labor Statistics, the Administration for Children and Families, and the House Ways and Means Committee.

NOTES: Data are for counties: Atlanta (Fulton County), Georgia; Grand Rapids (Kent County), Michigan; Riverside (Riverside County), California; Columbus (Franklin County), Ohio; Detroit (Wayne County), Michigan; Oklahoma City (Oklahoma, Cleveland, and Pottowatomie counties), Oklahoma; Portland (Multnomah and Washington counties), Oregon.

[a] Employment statistics and data were obtained from the Bureau of Labor Statistics Web site.

[b] Poverty rate and income estimates were obtained from the Census Bureau Web site.

[c] Welfare Caseload statistics are for states.

[d] Data are through March of 1998.

fer more opportunities for work and advancement than do high-poverty cities like Atlanta, Detroit, and Oklahoma City. Across the sites, median income is correlated with the poverty rate; median incomes were highest in Grand Rapids, Columbus, and Riverside and lowest in Oklahoma City and Detroit.

The bottom panel of Table 5.2 compares some welfare statistics across the seven sites. Caseload reductions are shown for the period 1995-1998 and were substantial in each of the six states in the study. Portland's caseload reduction was largest, at 54.8 percent, and Riverside's was smallest, at 21.9 percent—still a meaningful reduction. AFDC grants in 1996 varied substantially across the sites as well. Riverside's monthly maximum of $607 was the highest grant, and Atlanta's maximum of $280 was the lowest. Grant levels are important predictors of variation in welfare impacts, because higher grants enable working recipients to stay on welfare longer. (In low-grant states, even a small amount of earnings usually ends a recipient's eligibility to receive welfare.)[7] Thus, small increases in earnings in a low-grant state may cause substantial reductions in welfare receipt, while similar increases in a high-grant state would have little effect on welfare receipt.

The last measure presented in Table 5.2 shows the maximum AFDC grant as a percentage of median income. This is another measure of the relative size of the welfare grants in the various states, but it adjusts this comparison for the variation in median income (and possibly cost of living) across the states. Doing this somewhat reduces the apparent gap in grant levels between, for example, Oklahoma City and Riverside. All these measures will be revisited later, when we attempt to explain variation in program effects across the sites.

V. Program Effects: An Overview

Table 5.3 shows estimated program effects on three-year earnings and effects on earnings in the third year alone for each of the 11 programs in the NEWWS study. The table shows that impacts were substantial and positive for most of the programs and were sustained throughout the three-year follow-up period. Overall, NEWWS program group members who entered the study without a high school diploma or GED earned an average of $1,212 more than their counterparts in the control group. In the third year, the programs increased these sample members' earnings by $543. In relative terms, impacts for the third year alone (a gain of 21.8 percent for the full sample) were comparable to those for all three years combined (21.7 percent). The only programs that failed to significantly increase sample members' earnings were the ET&E program[8] in Oklahoma City, the Traditional program in Columbus (not featuring integrated case management), and the HCD program in Atlanta. Statistical tests (not shown here) confirmed that the cross-site variation in impacts was statistically significant. The source of this variation will be explored in greater detail in a later section.

Table 5.4 shows effects on AFDC receipt during the three years of follow-up. Reductions in AFDC receipt during that time were comparable to the increases in earnings shown in Table 5.3. Overall, sample members received $1,056 fewer AFDC benefits—a reduction of 10.6 percent. This

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[7] Under the new Temporary Assistance for Needy Families (TANF) program, increased earnings disregards in many states have made it easier for welfare recipients to combine welfare and work.

[8] The ET&E program is Oklahoma's Education, Training, and Employment program.

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Table 5.3

For Sample Members Without a High School Diploma or GED at Random Assignment: Program Impacts on Earnings in Year 3

Site and Program	Sample Size	Program Group	Control Group	Difference (Impact)	Percentage Change[a] (%)	p-Value
			Average Total Earnings in Years 1 to 3 ($)			
All 11 programs	18,326	6,797	5,585	1,212 ***	21.7	0.000
Atlanta Labor Force Attachment	1,495	6,475	5,391	1,083 **	20.1	0.022
Atlanta Human Capital Development	1,519	6,102	5,391	710	13.2	0.131
Grand Rapids Labor Force Attachment	1,251	7,607	5,198	2,409 ***	46.4	0.000
Grand Rapids Human Capital Development	1,209	6,126	5,198	928 **	17.9	0.049
Riverside Labor Force Attachment	3,125	6,674	5,285	1,389 ***	26.3	0.000
Riverside Human Capital Development	3,135	6,025	5,285	740 **	14.0	0.048
Columbus Integrated	1,987	9,938	8,208	1,730 ***	21.1	0.001
Columbus Traditional	2,001	8,942	8,208	734	8.9	0.139
Detroit	1,940	6,237	5,297	940 **	17.7	0.033
Oklahoma City	2,601	4,234	3,913	321	8.2	0.184
Portland	1,872	8,327	5,895	2,432 ***	41.2	0.000
			Average Total Earnings in Year 3 ($)			
All 11 programs	18,326	3,031	2,487	543 ***	21.8	0.000
Atlanta Labor Force Attachment	1,495	2,725	2,374	351	14.8	0.117
Atlanta Human Capital Development	1,519	2,668	2,374	294	12.4	0.186
Grand Rapids Labor Force Attachment	1,251	3,346	2,395	951 ***	39.7	0.000
Grand Rapids Human Capital Development	1,209	3,028	2,395	633 **	26.4	0.011
Riverside Labor Force Attachment	3,125	2,538	2,150	388 **	18.1	0.025
Riverside Human Capital Development	3,135	2,554	2,150	404 **	18.8	0.020
Columbus Integrated	1,987	4,328	3,590	738 ***	20.5	0.002
Columbus Traditional	2,001	3,821	3,590	231	6.4	0.330
Detroit	1,940	3,227	2,634	594 **	22.5	0.012
Oklahoma City	2,601	1,841	1,678	163	9.7	0.197
Portland	1,872	3,736	2,551	1,185 ***	46.5	0.000

(continued)

Table 5.3 (continued)

SOURCE: MDRC calculations from unemployment insurance (UI) earnings records.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Each site was weighted equally in the pooled estimates.

Estimates were regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members.

Rounding may cause slight discrepancies in calculating sums and differences.

A two-tailed t-test was applied to differences between outcomes for the program and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

[a]"Percentage change" equals 100 times the "difference" divided by "control group."

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Table 5.4

For Sample Members Without a High School Diploma or GED at Random Assignment: Program Impacts on AFDC Receipt and Payments

Site and Program	Sample Size	Program Group	Control Group	Difference (Impact)	Percentage Change[a] (%)	p-Value
			Average Total AFDC Payments Received in Years 1 to 3 ($)			
All 11 programs	18,326	8,917	9,972	-1,056 ***	-10.6	0.000
Atlanta Labor Force Attachment	1,495	7,121	7,569	-448 ***	-5.9	0.004
Atlanta Human Capital Development	1,519	7,193	7,569	-376 **	-5.0	0.015
Grand Rapids Labor Force Attachment	1,251	8,749	10,967	-2,218 ***	-20.2	0.000
Grand Rapids Human Capital Development	1,209	9,443	10,967	-1,524 ***	-13.9	0.000
Riverside Labor Force Attachment	3,125	12,125	14,166	-2,041 ***	-14.4	0.000
Riverside Human Capital Development	3,135	12,440	14,166	-1,725 ***	-12.2	0.000
Columbus Integrated	1,987	6,661	8,065	-1,404 ***	-17.4	0.000
Columbus Traditional	2,001	7,191	8,065	-874 ***	-10.8	0.000
Detroit	1,940	12,755	13,102	-347	-2.6	0.132
Oklahoma City	2,601	5,300	5,293	7	0.1	0.964
Portland	1,872	8,897	10,514	-1,617 ***	-15.4	0.000
			Average Total AFDC Payments Received in Year 3 ($)			
All 11 programs	18,326	2,313	2,703	-390 ***	-14.4	0.000
Atlanta Labor Force Attachment	1,495	1,971	2,123	-152 **	-7.2	0.034
Atlanta Human Capital Development	1,519	2,038	2,123	-85	-4.0	0.231
Grand Rapids Labor Force Attachment	1,251	2,336	2,981	-645 ***	-21.6	0.000
Grand Rapids Human Capital Development	1,209	2,434	2,981	-547 ***	-18.4	0.000
Riverside Labor Force Attachment	3,125	3,230	3,865	-635 ***	-16.4	0.000
Riverside Human Capital Development	3,135	3,189	3,865	-677 ***	-17.5	0.000
Columbus Integrated	1,987	1,467	1,999	-532 ***	-26.6	0.000
Columbus Traditional	2,001	1,666	1,999	-334 ***	-16.7	0.000
Detroit	1,940	3,584	3,809	-225 **	-5.9	0.037
Oklahoma City	2,601	1,389	1,369	20	1.5	0.747
Portland	1,872	2,052	2,729	-677 ***	-24.8	0.000

(continued)

Table 5.4 (continued)

SOURCE: MDRC calculations from AFDC records.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Each site was weighted equally in the pooled estimates.

Estimates were regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members.

Rounding may cause slight discrepancies in calculating sums and differences.

A two-tailed t-test was applied to differences between outcomes for the program and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

[a]"Percentage change" equals 100 times the "difference" divided by "control group."

impact also was sustained throughout the follow-up period. In the third year, welfare benefits were reduced by $390, or 14.4 percent. Again, cross-site variation in impacts was found to be statistically significant, with Oklahoma City and Detroit failing to show a reduction in three-year AFDC receipt, and Oklahoma City and Atlanta HCD not showing such a reduction in the third year. Both Riverside programs were the only ones among the 11 studied in which AFDC reductions far outweighed earnings gains, thus presumably reducing sample members' disposable income. In the other programs, earnings gains were comparable to or larger than the AFDC reductions.

In an attempt to begin explaining the cross-site variation in program effects, we grouped the sites according to the typology discussed in the introduction to this chapter. The HCD programs in Atlanta, Grand Rapids, and Riverside were grouped together with the two programs in Columbus to form a group of education-focused programs in which the participation mandate was strictly enforced. The three LFA programs in Atlanta, Grand Rapids, and Riverside were also characterized as high enforcement but were focused on employment instead of education, starting out their programs with a structured job search for each participant. Detroit and Oklahoma City were both education-focused but lacked strict enforcement of participation mandates; essentially, sample members who were unwilling to participate in these programs could drop out (or never show up) without serious consequences for their welfare grant. Finally, Portland is in a class of its own, combining strict enforcement of mandates with a flexible approach to the sequencing of activities. While the program was generally employment-focused, it allowed some participants to start their program experience with a basic education or vocational training class.

Tables 5.5 and 5.6 show results from analyses of impacts on earnings and AFDC receipt for these four groupings of programs. The pattern of earnings impacts is fairly straightforward. For all three years combined, Portland (employment focus, varied first activity, high enforcement) tops the list with a program effect of $2,433, or 40.4 percent, followed by the other employment-focused sites ($1,698), the education-focused sites with high enforcement ($944), and the two low enforcement sites ($569). In the third year, differences become less pronounced, with the exception of Portland's lead over the rest of the sites.

Looking at the AFDC impacts shown in Table 5.6, we see pronounced differences along the enforcement dimension (as might be expected, since that dimension also captures variation in sanctioning rates). The table shows that the two low enforcement sites did not significantly reduce AFDC receipt in the third year or in the three follow-up years combined.

VI. Analyzing Variation in Program Effects

A. Experimental Comparisons of Programs Within Sites

The next step in our analysis was to pay more attention to the cross-program differences shown in the previous four tables. First, we focused on the fully experimental comparisons, that is, comparisons involving multiple randomly assigned program groups in the same site or group of sites. Table 5.7 summarizes these comparisons, showing program effects accompanied by p-

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Table 5.5

For Sample Members Without a High School Diploma or GED at Random Assignment: Program Impacts on Earnings in Year 3, by Program Grouping

Program Approach	Sample Size	Program Group	Control Group	Difference (Impact)	Percentage Change[a] (%)	p-Value
			Average Total Earnings in Years 1 to 3 ($)			
All 11 programs	18,326	6,797	5,585	1,212 ***	21.7	0.000
Employment focus						
Job search first activity, high enforcement[b]	2,977	7,706	6,008	1,698 ***	28.3	0.000
Varied activity, high enforcement[c]	1,154	8,450	6,017	2,433 ***	40.4	0.000
Education focus						
High enforcement[d]	5,127	6,966	6,023	944 ***	15.7	0.000
Low enforcement[e]	2,267	6,678	6,110	569 **	9.3	0.028
			Average Total Earnings in Year 3 ($)			
All 11 programs	18,326	3,031	2,487	543 ***	21.8	0.000
Employment focus						
Job search first activity, high enforcement[b]	2,977	3,279	2,696	583 ***	21.6	0.000
Varied activity, high enforcement[c]	1,154	3,742	2,683	1,059 ***	39.5	0.000
Education focus						
High enforcement[d]	5,127	3,132	2,678	454 ***	16.9	0.000
Low enforcement[e]	2,267	3,074	2,707	367 ***	13.6	0.004

SOURCE: MDRC calculations from unemployment insurance (UI) records.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Each site was weighted equally in the pooled estimates.

Estimates were regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members.

Rounding may cause slight discrepancies in calculating sums and differences.

A two-tailed t-test was applied to differences between outcomes for the program and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

[a] "Percentage change" equals 100 times "difference" divided by "control group."

[b] This grouping includes the Labor Force Attachment programs in Atlanta, Grand Rapids, and Riverside.

[c] This "grouping" includes only the Portland, Oregon site.

[d] This grouping includes the Human Capital Development programs in Atlanta, Grand Rapids, and Riverside, and the Integrated and Traditional programs in Columbus, Ohio.

[e] This grouping includes the programs in Oklahoma City and Detroit.

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Table 5.6

For Sample Members Without a High School Diploma or GED at Random Assignment: Program Impacts on AFDC Receipt and Payments, by Program Approach

Program Approach	Sample Size	Program Group	Control Group	Difference (Impact)	Percentage Change[a] (%)	p-Value
		Average Total AFDC Payments Received in Years 1 to 3 ($)				
All 11 programs	18,326	8,917	9,972	-1,056 ***	-10.6	0.000
Employment focus						
Job search first activity, high enforcement[b]	2,977	8,058	9,612	-1,554 ***	-16.2	0.000
Varied activity, high enforcement[c]	1,154	7,763	9,574	-1,811 ***	-18.9	0.000
Education focus						
High enforcement[d]	5,127	8,483	9,653	-1,170 ***	-12.1	0.000
Low enforcement[e]	2,267	9,307	9,467	-160	-1.7	0.269
		Average Total AFDC Payments Received in Year 3 ($)				
All 11 programs	18,326	2,313	2,703	-390 ***	-14.4	0.000
Employment focus						
Job search first activity, high enforcement[b]	2,977	2,083	2,559	-476 ***	-18.6	0.000
Varied activity, high enforcement[c]	1,154	1,822	2,560	-739 ***	-28.8	0.000
Education focus						
High enforcement[d]	5,127	2,155	2,584	-429 ***	-16.6	0.000
Low enforcement[e]	2,267	2,420	2,522	-102	-4.0	0.104

SOURCE: MDRC calculations from unemployment insurance (UI) records.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Each site was weighted equally in the pooled estimates.

Estimates were regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members.

Rounding may cause slight discrepancies in calculating sums and differences.

A two-tailed t-test was applied to differences between outcomes for the program and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

[a] "Percentage change" equals 100 times "difference" divided by "control group."

[b] This grouping includes the Labor Force Attachment programs in Atlanta, Grand Rapids, and Riverside.

[c] This "grouping" includes only the Portland, Oregon site.

[d] This grouping includes the Human Capital Development programs in Atlanta, Grand Rapids, and Riverside, and the Integrated and Traditional programs in Columbus, Ohio.

[e] This grouping includes the programs in Oklahoma City and Detroit.

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Table 5.7

For Sample Members Without a High School Diploma or GED at Random Assignment: Experimental Comparisons of Different Program Approaches

Program Approach	Sample Size	Program Effect on: Three-Year Earnings ($)	Year 3 Earnings ($)	Three-Year AFDC ($)	Year 3 AFDC ($)
Atlanta					
Labor Force Attachment	1,495	1,083 **	351	-448 ***	-152 **
Human Capital Development	1,519	710	294	-376 **	-85
p-value for difference between effects[a]		*0.390*	*0.729*	*0.804*	*0.588*
Grand Rapids					
Labor Force Attachment	1,251	2,409 ***	951 ***	-2,218 ***	-645 ***
Human Capital Development	1,209	928 **	633 **	-1,524 ***	-547 ***
p-value for difference between effects[a]		*0.005* ***	*0.203*	*0.012* **	*0.408*
Riverside					
Labor Force Attachment	3,125	1,389 ***	388 **	-2,041 ***	-635 ***
Human Capital Development	3,135	740 **	404 **	-1,725 ***	-677 ***
p-value for difference between effects[a]		*0.210*	*0.950*	*0.313*	*0.738*
Columbus					
Integrated case management	1,987	1,730 ***	738 ***	-1,404 ***	-532 ***
Traditional	2,001	734	231	-874 ***	-334 ***
p-value for difference between effects[a]		*0.071* *	*0.070* *	*0.056* *	*0.090* *

SOURCE: MDRC calculations from unemployment insurance (UI) records and AFDC records.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Each site was weighted equally in the pooled estimates.

Estimates were regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members.

A two-tailed t-test was applied to differences between outcomes for the program and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

[a] This p-value is obtained from an F-test assessing the statistical significance of differences in impacts between different program options in the same site.

values for differences across the different programs within the same site.[9] From the table it appears that the contrast between Human Capital Development and Labor Force Attachment approaches was statistically significant only in Grand Rapids, and only during the first two years of follow-up in that site. During those first two years, Grand Rapids' LFA program was significantly more successful than its HCD program, but this was no longer the case in year 3.

In their analysis of two-year findings for the full NEWWS sample in these three sites, Hamilton et al. found much larger differences in favor of the LFA approach.[10] Unsurprisingly, this suggests that a Human Capital Development approach may hold more promise for sample members who do not already have a high school diploma or GED. However, Table 5.7 shows no evidence that an HCD approach works better for these sample members than an LFA approach, and even nonsignificant differences in impacts continue to favor LFA programs, even for those entering the programs without a high school diploma or GED. Also, expectations that HCD programs would catch up with LFA programs after several years were not confirmed, at least not by the third year of follow-up.

The other fully experimental comparison of program options, carried out in Columbus, explored the effects of different case management options. Although differences were not overwhelming, this comparison consistently favored the integrated case management approach, finding it to be more effective for sample members without a high school diploma or GED than a more traditional approach to case management. Differences in impacts across these two options were always statistically significant—at the 10 percent level at least. This contrast in program effects is stronger than that reported by Brock and Harknett for the full sample (which included those entering the program with a high school diploma or GED).[11]

B. Comparing Program Effects While Adjusting for Cross-Site Differences in Sample Characteristics

Next, we focused on the other cross-program comparisons in an attempt to identify the effects of enforcement, variation in first activity, and other cross-site differences. However, because these comparisons were not based on random assignment to different program options, it was necessary to control for cross-site differences in sample characteristics, as outlined in the introduction to this chapter. The result of implementing such an adjustment is a set of hypothetical impact estimates that would have been found if all programs served the same population, whose characteristics would represent the average across all sample members in the seven NEWWS sites. In other words, if one program served a more "hard-to-serve" population than another, the new estimates would adjust that program's impacts accordingly. The result is that program effects are more similar across the sites, reducing the variation that is ultimately attributed to differences in program characteristics.

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[9] Such p-values show the probability that estimates are [illegible] across [illegible]. When such a probability drops below 10 percent, the differences are considered statistically significant, as indicated with stars or daggers.

[10] Hamilton et al., 1997.

[11] Brock and Harknett, 1998.

Table 5.8 shows such adjusted program effects for each of the 11 programs and also shows the results of a number of tests applied to the differences across the programs. Note that these tests not only capture the effects of differences in program approach but also capture any other differences that were not controlled for when we adjusted the estimates for the sample composition. Such differences include the program environment, the economy, and any individual characteristics that were unmeasured.

The table shows that the adjustment procedure improved program effects in Atlanta and Detroit (compared with earlier tables). It reduced program effects in Riverside and, to a lesser extent, in Oklahoma City. This suggests that the latter two sites served welfare recipients who were expected to benefit more from participation in a welfare-to-work program. (It does not mean that these sample members are less disadvantaged or in need of less help from the program.) After the adjustment for sample composition, some dimensions capturing program differences were no longer statistically significant, but the general pattern of program effects was unchanged: high enforcement programs had greater impacts than low enforcement programs (especially on AFDC receipt); Grand Rapids' LFA program had significantly greater impacts than Atlanta's or Riverside's; and Portland still had larger program effects than most of the other sites, especially in the third year of follow-up.

Comparing the impacts shown at the bottom of Table 5.8 for the four program groupings with those presented in Tables 5.5 and 5.6, we see that the effects of adjusting for individual characteristics was modest for these groupings. This reflects the more heterogeneous character of groupings as compared with individual sites. (For example, the "education focus, high enforcement" group included sample members from Atlanta, Grand Rapids, Riverside, and Columbus.) The pattern of impacts across the groupings remained unchanged and statistically significant.

C. Attributing Variation to Environmental Factors and Program Characteristics

In a final step to explain variation in program effects, we conducted an analysis in which we regressed the adjusted program effects shown in Table 5.8 on nine different characteristics of the programs and their environment. The program variables in this analysis included (1) whether or not the program was education-focused (as opposed to employment-focused), (2) the program's enforcement level, (3) whether the program allowed different first activities (this distinguished Portland from the other employment-focused sites), and (4) whether it had integrated case management. Among environmental variables, we included (1) the 1996 grant level, (2) overall levels of caseload reduction since 1995, (3) the 1995 estimated poverty rate, (4) job growth between 1996 and 1997, and (5) the 1996 unemployment rate. The statistical power of this analysis was limited, because it compared only 11 programs, estimating the contribution of nine different predictor variables. This does not leave many degrees of freedom. Nevertheless, a comparative analysis like this can give an indication of which site-specific characteristics might matter most.

Figure 5.3 summarizes the results of this analysis. Each of the horizontal bars in the figure shows how an environmental or programmatic factor "affects" the impact of a welfare-to-work program on the four outcomes featured throughout this chapter (total earnings in years 1-3, earnings in year 3, amount of AFDC received in years 1-3, and amount of AFDC received in year 3). Thus, each horizontal bar captures the size and direction (positive or negative) of one of the

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Table 5.8

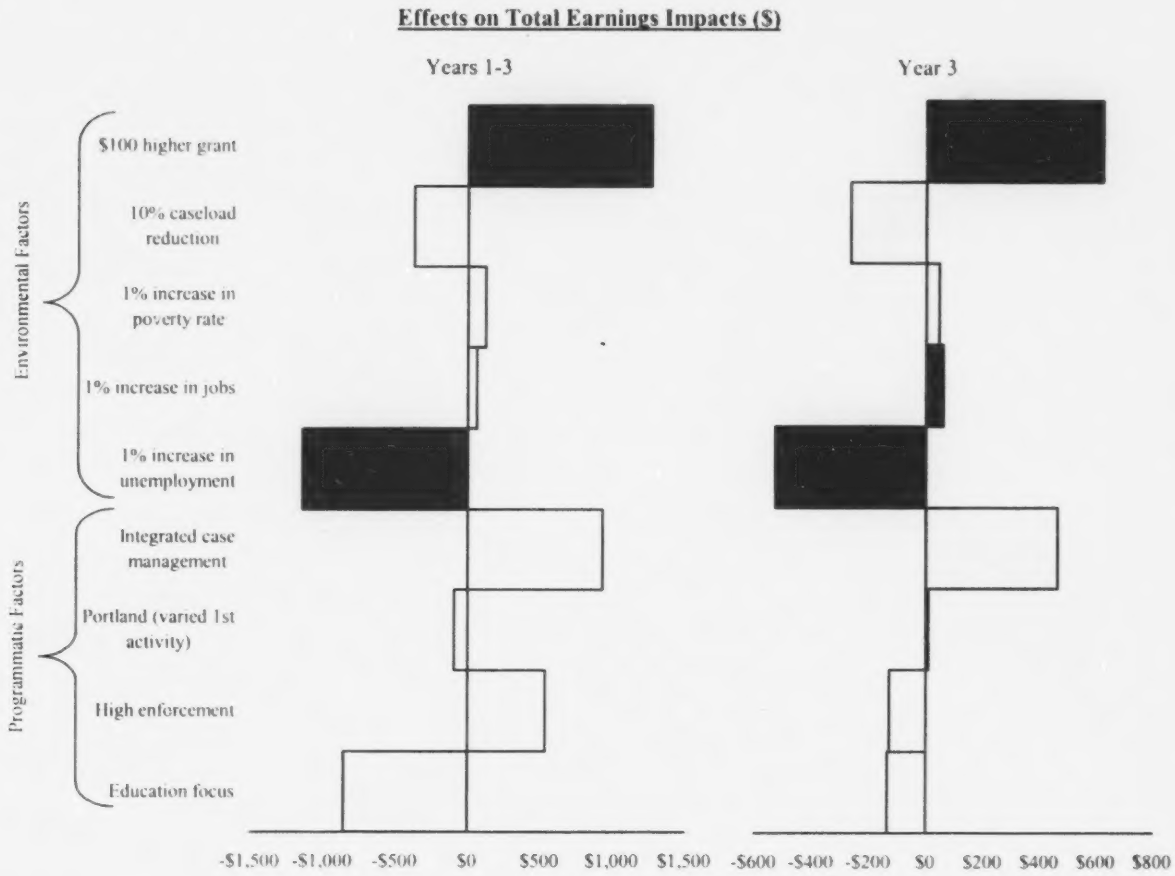
For Sample Members Without a High School Diploma or GED at Random Assignment: Comparing Program Effects Across 11 Programs, Statistically Adjusted for Variation in Sample Composition Across the Sites

		Statistically Adjusted Estimate of Program Effect on:			
Program Approach	Sample Size	Three-Year Earnings ($)	Year 3 Earnings ($)	Three-Year AFDC ($)	Year 3 AFDC ($)
Atlanta					
Labor Force Attachment	1,495	1,635 **	508	-642 *	-39
Human Capital Development	1,519	1,189 *	421	-567	28
Grand Rapids					
Labor Force Attachment	1,251	2,559 ***	955 ***	-2,197 ***	-494 ***
Human Capital Development	1,209	1,100 *	628 **	-1,474 ***	-390 ***
Riverside					
Labor Force Attachment	3,125	839	114	-1,767 ***	-383 **
Human Capital Development	3,135	175	124	-1,469 ***	-421 ***
Columbus					
Integrated case management	1,987	1,908 ***	710 **	-1,519 ***	-435 ***
Traditional	2,001	976	246	-952 ***	-221
Detroit	1,940	1,392 **	715 **	-508	-114
Oklahoma City	2,601	227	94	80	138
Portland	1,872	2,480 ***	998 ***	-1,804 ***	-602 ***
p-value for all site difference[a]		*0.000* ***	*0.011* **	*0.000* ***	*0.000* ***
Job search, high enforcement	5,871	1,428 ***	367	-1,484 ***	-281 **
Varied first activity, high enforcement[b]	1,872	2,193 ***	848 ***	1,693 ***	-551 ***
Education, high enforcement	9,851	692	244	-1,125 ***	-249 **
Education, low enforcement	4,541	525	262	-211	19
p-value for all approaches[a]		*0.000* ***	*0.035* **	*0.000* ***	*0.000* ***
p-value for first activity[a]		*0.096* *	*0.034* **	*0.417*	*0.015* **
p-value for enforcement[a]		*0.635*	*0.918*	*0.000* ***	*0.002* ***

(continued)

Figure 5.3

For Sample Members Without a High School Diploma or GED at Random Assignment: How Different Programmatic and Environmental Factors Relate to Program Effects on Earnings and the Amount of AFDC Received



Statistical Significance of Relationships:

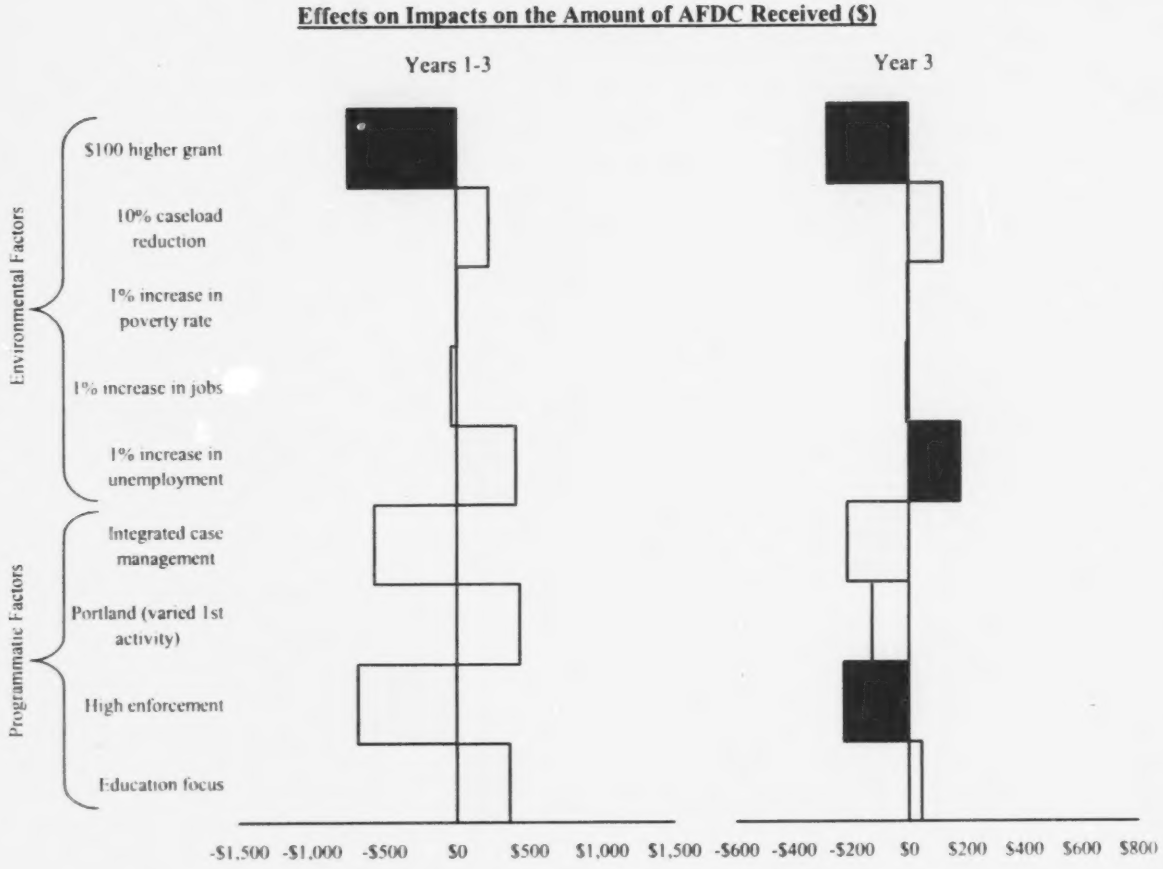
■ Statistically significant (p-value less than 0.10)

☐ Marginally statistically significant (p-value between 0.10 and 0.20)

☐ Not statistically significant

(continued)

Figure 5.3 (continued)



Statistical Significance of Relationships:

- ■ Statistically significant (p-value less than 0.10)
- ☐ Marginally statistically significant (p-value between 0.10 and 0.20)
- ☐ Not statistically significant

SOURCES: MDRC calculations from unemployment insurance (UI) records, AFDC records, and published data on the labor market and welfare environment in the NEWWS sites (see Table 5.2 for details).

NOTES: These estimates predict variation in statistically adjusted impacts as presented in Table 5.8. Because of the small sample size (11 observations), these estimates are not highly reliable.

thin vertical arrows shown in Figure 5.2. The black bars identify statistically significant "effects" ($p<0.10$), while the gray bars identify marginally significant ones ($p<0.20$). White bars should probably be ignored, as they merely represent random variation in the sample.

Figure 5.3 suggests an interesting pattern of effects. First, program effects on all four outcomes were strongly influenced by the grant level in the state in which the program operated. A hypothetical increase of $100, holding other factors constant, might predict a $1,200 increase in three-year earnings impacts, a $600 increase in such impacts for the third year alone, and sizable differences in program effects on AFDC receipt. This finding is important, because it suggests that, even controlling for sample characteristics and economic circumstances, welfare-to-work programs may have different effects depending on the relative generosity of the overall welfare system. Programs working within the context of a more generous system have greater potential to change sample members' behavior, choices, and outcomes, which may explain part of these differences.

In addition to grant levels, local unemployment seems to be an important predictor of program effects on earnings. Again, controlling for individual caseload characteristics and program characteristics, program effects on earnings appear to have been dampened by higher unemployment, which also was reflected (but less strongly) in program effects on welfare receipt. In our data, much of this effect may have been driven by Riverside, which had substantially higher unemployment rates than other sites; the generalizability of this finding may thus be limited.

Other interesting findings include the fact that caseload reductions were negatively correlated with program success. This makes sense, given that such reductions reflect change in the larger welfare system, which affects both program and control group members, thereby reducing the potential size of welfare-to-work program effects.

Compared with welfare grant levels and unemployment rates, welfare-to-work program characteristics were relatively less significant predictors of program effects. Regarding the impacts on earnings, it appears that an education focus as opposed to an employment focus reduced impacts during the first two years but not during the third year (similar to what we found earlier). Education-focused programs also had smaller AFDC savings during the first two years. Other interesting findings include the apparent benefits of integrated case management, which increased earnings and AFDC impacts during the third year of follow-up, and the fact that high enforcement programs had larger AFDC impacts, probably due to higher rates of sanctioning and more exits from welfare.

VII. How Program Effects Varied for Subgroups

We conclude with a closer examination of the importance of specific individual characteristics in predicting program success. For this purpose, Tables 5.9 and 5.10 present program effects for 18 subgroups defined along seven dimensions, which also were used in Chapter 2. Our discussion of program effects on earnings and AFDC receipt for these subgroups focuses on dimensions across which there were statistically significant differences in the impacts (indicated with daggers in the tables).

In Table 5.9, for example, we see such a difference in impacts for sample members with different initial reading levels. In particular, early impacts on earnings (during the first two years

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Table 5.9

For Sample Members Without a High School Diploma or GED at Random Assignment: Three-Year Impacts on Earnings, Pooled Across Three Education-Focused Programs, by Selected Characteristics at the Time of Random Assignment

		Total Earnings, Year 1-3 ($)			Earnings Year 3 ($)		
Subgroup	Sample Size	Program Group	Control Group	Difference (Impact)	Program Group	Control Group	Difference (Impact)
Score on TALS document literacy test[a]				†			
Level 1 - 2	4,319	5,702	4,652	1,050 ***	2,447	2,047	400 ***
Level 3 - 5	2,797	8,394	6,545	1,849 ***	3,615	2,868	747 ***
Highest grade completed in school							
8 or below	2,443	5,622	4,323	1,299 ***	2,433	1,914	519 ***
9 - 10	7,328	6,499	5,138	1,360 ***	2,911	2,314	597 ***
11 or above	8,385	7,416	6,391	1,025 ***	3,312	2,835	476 ***
Level of economic disadvantage[b]							
More disadvantaged	7,723	4,549	3,250	1,299 ***	2,267	1,643	624 ***
Less disadvantaged	10,511	8,526	7,324	1,202 ***	3,625	3,112	513 ***
Age of youngest child							
5 or under	10,670	6,354	5,033	1,322 ***	2,917	2,279	637 ***
6 or over	7,561	7,500	6,404	1,097 ***	3,218	2,798	420 ***
Reported barriers to participation[c]				†††			†††
Many family or personal problems *and* parental concerns	1,038	4,737	2,967	1,771 ***	2,040	1,416	624 ***
Many family or personal problems	544	4,583	4,958	-375	2,020	2,210	-190
Many parental concerns	1,344	6,942	4,204	2,738 ***	3,243	2,043	1,200 ***
Neither barrier	4,349	8,608	6,548	2,060 ***	3,717	2,832	885 ***
Preference for school[c]							
Do not like and/or do not plan to attend school	2,818	7,382	5,506	1,875 ***	3,204	2,472	732 ***
Like and/or plan to attend school	4,860	7,436	5,463	1,973 ***	3,274	2,418	856 ***
Depressive symptoms[c]				††			
Many symptoms	1,076	6,948	5,909	1,039 *	2,965	2,445	520 *
Moderate number of symptoms	1,776	7,097	5,919	1,178 **	3,172	2,660	511 **
Few symptoms	4,264	7,791	5,328	2,463 ***	3,408	2,417	991 ***

(continued)

Table 5.9 (continued)

SOURCES: MDRC calculations from information routinely collected by welfare staff, unemployment insurance (UI) earnings records, TALS document literacy test and CASAS math test data, and Private Opinion Survey data.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Each site was weighted equally in the pooled estimates.

Estimates were regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members.

Sample sizes for individual measures vary because of missing values.

Rounding may cause slight discrepancies in calculating sums and differences.

A two-tailed t-test was applied to differences between outcomes for the program and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

An F-test was applied to differences among subgroups for each characteristic. Statistical significance levels are indicated as: † = 10 percent; †† = 5 percent; and ††† = 1 percent.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

[a] TALS scores for Riverside are based on scores earned on the Greater Avenues for Independence (GAIN) Appraisal literacy test and are converted to their TALS equivalent.

[b] "More disadvantaged" individuals are those who did not work for pay in the year prior to random assignment and who received AFDC for more than two years prior to random assignment. The "less disadvantaged" category contains those who did not meet these criteria.

[c] These subgroups are based on scales created from Private Opinion Survey data. For an explanation of these subgroups see Appendix E.

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Table 5.10

For Sample Members Without a High School Diploma or GED at Random Assignment: Three-Year Impacts on AFDC Receipt, Pooled Across Three Education-Focused Programs, by Selected Characteristics at the Time of Random Assignment

		Total AFDC, Year 1-3 ($)			AFDC Year 3 ($)		
Subgroup	Sample Size	Program Group	Control Group	Difference (Impact)	Program Group	Control Group	Difference (Impact)
Score on TALS document literacy test[a]							
Level 1 - 2	4,319	9,587	10,982	-1,395 ***	2,586	3,020	-434 ***
Level 3 - 5	2,797	9,454	10,764	-1,310 ***	2,463	2,881	-418 ***
Highest grade completed in school							
8 or below	2,443	8,779	9,644	-865 ***	2,241	2,557	-317 ***
9 - 10	7,328	8,719	9,825	-1,106 ***	2,244	2,675	-431 ***
11 or above	8,385	9,134	10,166	-1,031 ***	2,395	2,755	-360 ***
Level of economic disadvantage[b]							
More disadvantaged	7,723	10,709	11,855	-1,146 ***	2,849	3,278	-429 ***
Less disadvantaged	10,511	7,586	8,539	-954 ***	1,913	2,266	-354 ***
Age of youngest child				†			
5 or under	10,670	9,922	10,870	-948 ***	2,679	3,056	-377 ***
6 or over	7,561	7,419	8,629	-1,210 ***	1,765	2,173	-408 ***
Reported barriers to participation[c]							
Many family or personal problems *and* parental concerns	1,038	10,658	12,015	† -1,357 ***	2,903	3,372	†† -469 ***
Many family or personal problems	544	9,232	10,104	-872 *	2,429	2,671	-242
Many parental concerns	1,344	9,622	11,886	-2,264 ***	2,380	3,299	-919 ***
Neither barrier	4,349	8,461	9,889	-1,427 ***	2,140	2,616	-475 ***
Preference for school[c]							
Do not like and/or do not plan to attend school	2,818	8,823	10,623	-1,800 ***	2,271	2,848	-577 ***
Like and/or plan to attend school	4,860	9,156	10,532	-1,376 ***	2,344	2,843	-499 ***
Depressive symptoms[c]							
Many symptoms	1,076	8,902	9,900	-998 ***	2,217	2,471	-254 *
Moderate number of symptoms	1,776	9,178	10,479	-1,301 ***	2,392	2,952	-560 ***
Few symptoms	4,264	9,016	10,597	-1,581 ***	2,305	2,834	-529 ***

(continued)

Table 5.10 (continued)

SOURCES: MDRC calculations from information routinely collected by welfare staff, AFDC records, TALS document literacy test and CASAS math test data, and Private Opinion Survey data.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Each site was weighted equally in the pooled estimates.

Estimates were regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members.

Sample sizes for individual measures vary because of missing values.

Rounding may cause slight discrepancies in calculating sums and differences.

A two-tailed t-test was applied to differences between outcomes for the program and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

An F-test was applied to differences among subgroups for each characteristic. Statistical significance levels are indicated as: † = 10 percent; †† = 5 percent; and ††† = 1 percent.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

[a] TALS scores for Riverside are based on scores earned on the Greater Avenues for Independence (GAIN) Appraisal literacy test and are converted to their TALS equivalent.

[b] "More disadvantaged" individuals are those who did not work for pay in the year prior to random assignment and who received AFDC for more than two years prior to random assignment. The "less disadvantaged" category contains those who did not meet these criteria.

[c] These subgroups are based on scales created from Private Opinion Survey data. For an explanation of these subgroups see Appendix E.

after random assignment) were stronger for those with higher initial reading levels. This is consistent with earlier findings that these sample members were more likely to receive a GED and also were somewhat less likely to participate in basic education for long periods of time, which would have depressed their earnings during the early part of the follow-up period. (This pattern was not reflected in impacts on AFDC receipt.)

The next subgroup dimension showing significant variation in impacts concerned sample members' self-reported barriers to participation. The Private Opinion Survey—administered to sample members in Atlanta, Grand Rapids, Riverside, and Portland—distinguished between barriers characterized as family and personal problems and barriers stemming from parental concerns about going to work or attending school (for example, fear about placing a child in daycare). Apparently, the programs were more successful in overcoming the latter barrier (perhaps by providing child care assistance or by reassuring participants that good child care would benefit their children). Thus, the strongest impacts occurred for those with many parental concerns; we found no impacts for those with many reported family and personal problems. Strangely, the impacts suggest that the presence of both kinds of barriers seemed to make sample members more like the group with many parental concerns than like the group with other barriers—possibly because, in those cases, the two kinds of barriers were related. Nevertheless, it is notable that among all subgroups studied, only those with many reported family or personal problems did not benefit from the programs in terms of increased earnings.

Another important predictor of program effects on earnings (but not on AFDC receipt) was sample members' emotional health as they entered the study. Those with few symptoms of depression were predicted to have earnings impacts more than twice as large as those having a moderate number or many of these symptoms. Comparing impacts on earnings and AFDC for the three subgroups defined by their depressive symptoms, it appears that only those with few symptoms were predicted to benefit financially from their participation in the welfare-to-work program. For the other two groups, AFDC reductions were as large as or larger than their earnings gains.

Finally, impacts on AFDC were significantly greater for sample members whose youngest child was 6 or older, especially during their first two years in the study. This makes sense, because single parents of older children were less likely to be exempted or excused from program participation—for example, when no appropriate child care is available.

VIII. Conclusions

The findings presented in this chapter show that welfare-to-work programs generally succeed in increasing earnings and reducing welfare dependency for sample members who enter these programs without a high school credential. In general, the magnitude of program effects on these outcomes was substantial, and impacts were not dramatically different from impacts reported in other publications for those who were not in need of basic education.

Within this general picture, however, there is substantial variation. Some variation reflects differences in other characteristics of the welfare recipients served by these programs, some is accounted for by differences in welfare grants and economic circumstances, and some reflects differences in program approaches. After controlling for individual characteristics and the program environment, we can cautiously conclude that (1) education-focused programs achieve smaller impacts on earnings during the early years of program participation, (2) programs with high levels of enforcement have stronger effects on AFDC receipt, and (3) programs with integrated case management have stronger impacts than programs with a more traditional separation of income maintenance and welfare-to-work tasks. In addition, we found some evidence that grant levels and unemployment are independently related to measured program effectiveness: higher grants translate into larger program effects, and higher unemployment reduces those effects. As far as individual characteristics were concerned, we found evidence that lower initial reading skills limit (or, at least, delay) program effects on earnings. We also found that personal barriers and emotional problems negatively affect participants' ability to benefit from welfare-to-work programs.

Chapter 6

Beyond Basic Education: The Benefits of Skills Training and College

I. Introduction

The services provided by welfare-to-work programs to welfare recipients who do not have a high school diploma or General Educational Development (GED) certificate are not limited to adult education (for example, adult basic education, GED preparation, or English as a Second Language classes). After completing a spell of adult education, participants in welfare-to-work programs may be referred to postsecondary skills training and college programs. Others may not receive adult education and may pursue skills training or college directly. Most training and college programs require enrollees to have completed their secondary school credential.[1] However, site visits showed that some programs will waive this requirement when a prospective enrollee successfully passes an interview or admissions test. Other programs require students who do not have a high school diploma or GED to take remedial classes in reading, writing, and math as a condition for their admission.

Little is known about the benefits of skills training and college programs for welfare recipients with low basic skills.[2] In published research, most participants in these postsecondary activities have graduated from high school. For example, in the evaluation of California's GAIN program, 42.8 percent of program group members with a high school diploma or GED participated in vocational training or postsecondary education, as opposed to 17.8 percent of those entering the program without a diploma or GED.[3] In the NEWWS Evaluation, Hamilton et al. report participation rates in vocational training of 16.7, 22.1, and 6.1 percent in Atlanta, Grand Rapids, and Riverside, respectively, for those entering the programs with a high school diploma or GED.[4] For those without such a credential, the corresponding participation rates were 5.2, 5.4, and 3.4 percent for these three sites, respectively.[5] Thus, relatively few welfare recipients lacking a high school diploma or GED receive skills training or college programs within the first several years following their enrollment in welfare-to-work or employment training programs. This is true even though many of these welfare recipients participate in adult basic education or GED preparation, activities that are supposed to prepare them for employment and are expected to provide students with the skills and credentials needed to enter job-focused training and college programs.

All this is of concern to policymakers because research suggests that postsecondary skills training and college programs are an effective way to improve the earnings and employment prospects of high school dropouts.[6] Postsecondary programs may be beneficial for several reasons. First, these programs augment the basic skills taught in adult education classes, skills that by themselves might not allow welfare recipients to become independent of public assistance. Second, the skills learned in postsecondary programs are often designed to meet the needs of employers, and the programs seek to match those needs with the interests of their students. This is reflected in the programs' curricula, which often feature hands-on instruction and may be complemented with internships. In addition, many training programs provide job development and job placement services. Provided the skills being taught are in demand in the labor market, a skills training program can fairly easily incorporate valuable brokerage services, supplying employers with qualified job candidates and supplying students with desirable jobs. A well-known example of a training program that is successful in matching prospective students to their future employers is the Center for Education and Training (CET), which is headquartered in San Jose, California. This program stands out for its success with individuals who are considered difficult to serve, including those without high school credentials and those with limited English skills.[7]

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[1] Murnane et al., 1994.

[2] In this chapter, we did not distinguish between vocational skills training and other postsecondary education, partly because the sample was too small to do so reliably, and partly because it often is difficult to positively identify a training program as being either a "college" or a "vocational program." Many of the latter have the word "college" in their name, and many "traditional" colleges offer vocational training to students who are not enrolled in the college itself.

[3] Riccio et al., 1994, pp. 44-45.

[4] Hamilton et al., 1997, p. 110.

[5] Hamilton et al., 1997, p. 110.

In response to findings that favor postsecondary programs, it might be expected that education programs for welfare recipients who lack a high school credential would actively seek to increase their students' access to postsecondary education and training programs. However, increased participation in such postsecondary services clashes with the immediate program goal of many welfare-to-work programs, which is a quick transition of welfare recipients from welfare into work. Since the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, the adoption of time limits on welfare receipt in many states has increased the pressure on welfare recipients and welfare-to-work programs to favor quick employment over continued participation in postsecondary training.

For welfare recipients in need of basic education, a focus on quick job entry often means that their education and training opportunities are limited and truncated. For example, in the 11 programs in the NEWWS Evaluation, sample members who did not have a high school diploma or GED spent an average of 14.4 weeks in education or training, or only 1.3 weeks longer than those who had graduated from high school before entering the study. Among welfare recipients who entered the programs without a high school diploma and who participated in adult basic education or GED preparation, only 14.9 percent also received skills training or enrolled in college within two years covered by a NEWWS follow-up survey. (More detail on these participation patterns is provided later in this chapter.) Therefore, it seems that the educational program objective of education-focused welfare-to-work programs is different depending on the educational skill levels with which welfare recipients enter the programs. Many of those entering with a high school diploma or GED leave having learned specific job skills, while most welfare recipients entering with low basic skills may improve their reading or math skills but are less likely to have specific work skills when they leave the programs. This could explain why, in terms of employment outcomes, education-focused welfare-to-work programs are sometimes more successful with high school graduates than with dropouts (as evidenced, for example, in Alameda County in the GAIN evaluation or in Atlanta in the NEWWS Evaluation).[8]

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[6] Murnane et al., 1995.

[7] Cave et al., 1993.

In this chapter we focus specifically on the experiences of welfare recipients who enter welfare-to-work programs with low basic skill levels and who enroll in adult education programs. Some of these participants subsequently make it to postsecondary education and training, but most do not. We set out to answer two general questions: (1) Who among adult education participants goes on to postsecondary education and training? and (2) How does enrollment in postsecondary programs affect their earnings and welfare receipt? Specifically, we describe participation patterns in skills training among welfare recipients who received adult education, and we explore the effects of such training for these sample members' longer-term (three-year) employment and welfare outcomes. Along those lines, this chapter is structured as follows. A summary of the findings is provided in section II, followed by a description of training-related participation patterns in section III, and an analysis of the effects of skills training and college in section IV. Section V concludes this chapter and develops policy implications.

In this chapter, as we did in some preceding ones, we use nonexperimental research methods that go beyond the experimental framework underlying the NEWWS Evaluation. In all analyses, we combine program and control group members across all 11 programs in the NEWWS study. In our analyses, we control for program status (for example, assignment to welfare-to-work programs or a control group) wherever possible. To maximize the utility of the available data, samples and data sources sometimes change as we move from one analysis to the next. Through all of this we seek to develop a comprehensive picture of training and college participation and to assess their benefits for welfare recipients with low initial basic skill levels.

II. Summary of the Findings

- **By the end of a two-year follow-up period, few sample members participating in adult education had enrolled in postsecondary education or training.**

This chapter begins by describing who, among adult education participants, continued on to enroll in postsecondary skills training and college programs. Only 14.9 percent of adult education participants received such postsecondary services. Among these participants, those with higher initial literacy levels and fewer missed high school years were more likely to make it to postsecondary programs.

- **Those who enrolled in postsecondary education or training programs were likely to have earned a GED during the follow-up period. They also spent relatively little time in adult education.**

Shorter stays in adult education and successful completion with a GED both were important predictors of subsequent enrollment in postsecondary activities. Receipt of a GED was predicted to increase enrollment in skills training or college by at least 27.7 percent. (It is possible, however, that GED receipt partly represented uncontrolled underlying differences in characteristics and motivation between those who earned the credential and those who did not. Also, a GED may have been required for admittance to many postsecondary programs.) Similarly, being in an adult education program for more than a year, as opposed to fewer than six months, was predicted to reduce one's chances of subsequently enrolling in skills training or college by 7.5 percent, possibly because welfare-to-work programs may limit the overall amount of time the participants spend on education or training.

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[8] See Riccio et al., 1994, pp. 137-138; and Hamilton et al., 1997, pp. 241 and 245.

- **Participation in postsecondary training or college was associated with higher earnings and greater self-sufficiency. However, it took several years for these effects to materialize.**

In the long run, those who participated in postsecondary programs did considerably better than those who did not participate in such programs. With appropriate caveats (that these are nonexperimental analyses and are subject to various potential biases), we found that postsecondary participants appear to earn more than those who received only adult education. In the third year following their last quarter of adult education, postsecondary participants were estimated to have earned $4,802, or $1,542 more than those receiving only adult education. This is a 47 percent increase. During the same year, postsecondary participants received $1,942 in welfare payments, which was $919 (or 32 percent) less than received by those who participated only in adult education. In the first two years following the last spell of adult education, no differences in earnings or welfare receipt were found. Initially, the earnings of participants in skills training or college programs would have been lowered by their participation in these programs (by making it more difficult for them to work). Even after participation in skills training or college ended, increases in earnings that reflected higher skills or training credentials took time to materialize, possibly because those who received training needed time to establish themselves in the labor market. This underscores the importance of longer-term follow-up to establish the payoff to education and training for welfare recipients entering education programs without a high school diploma.

III. Patterns of Participation in Postsecondary Skills Training and College Programs

A. Sample Definition

In this chapter, as in the entire report, our sample initially was limited to welfare recipients in the NEWWS study who did not have a high school diploma or GED when they entered the study (elsewhere referred to as the "Adult Basic Education," or "ABE," sample). To better understand the links between adult education and participation in postsecondary education, we further restricted the sample in this chapter, including only those who reported participating in adult education (ABE, GED preparation, or high school programs) at any point during the two-year follow-up period.[9] Thus, our sample includes 38.5 percent of program group members in the full ABE sample and 17.6 percent of control group members in that sample. The overall sample size is 1,330, but this sample is smaller for certain analyses that incorporate baseline test scores, describe long-term follow-up data on earnings and welfare receipt, or meet other analytical restrictions. It is important to acknowledge that this definition of the sample limits the extent to which our findings can be generalized. For example, our findings may not apply to welfare recipients who enter welfare-to-work programs already holding a high school credential.

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[9] This follow-up period is about 25 months long, but will often be referred to as a "two-year" period.

B. Who Received Postsecondary Services?

Among these 1,330 participants in adult education activities, only 198 (14.9 percent) went on to participate in postsecondary education or training (during the two-year follow-up period). Table 6.1 describes differences between those who enrolled in postsecondary education or training and those who did not.[10] Statistically significant differences between these two groups are indicated with asterisks. (The p-values shown in the table's third column indicate the probability that the apparent difference between the first two columns is really zero. The smaller this probability, the more meaningful the difference.)

The table's first panel shows how those who entered postsecondary education or training spent less time in *adult* education. Specifically, postsecondary participants were less likely to have been in adult education for more than a year. Only 8.3 percent spent more than a year in adult education, compared to 14.2 percent of those who did not receive postsecondary services. Overall, this is reflected in an average length of stay in adult education that was almost a month shorter for those who went on to postsecondary education or training than for those who did not. More substantial than the differences in length of stay in adult education was the difference in GED receipt between the two groups. Whereas 53.2 percent of those attending postsecondary education or training had a GED by the end of follow-up, only 12.7 percent of those in the adult education-only group did. This suggests that failure to earn a GED may be an important impediment to subsequent enrollment in postsecondary education or training (or, conversely, that getting such a credential is a strong incentive to enroll). On the other hand, 46.8 percent of postsecondary participants did *not* earn a GED, illustrating how access to such programs is possible without such a credential. Also, one has to consider the possibility that the GED did not actually contribute as much to postsecondary enrollment because unmeasured differences in individual characteristics could have caused part of the apparent differences in enrollment. In that case, sample members who received a GED and enrolled in skills training would have enrolled even if they had not been given an opportunity to earn a GED.

Table 6.1 also shows how the two groups compared on selected baseline characteristics, collected when these welfare recipients entered the NEWWS Evaluation. Most notably, those participating in postsecondary education or training had completed more grades in their regular school experience. On average, postsecondary participants had completed 10.1 grades when they entered the study, compared to 9.6 grades for the other group. Most of this difference was concentrated at the low and at the high end of the grade distribution: postsecondary participants were significantly less likely to have less than a 9th grade education coming into the study, and they were significantly *more* likely to have dropped out as late as 12th grade. Only 5.7 percent of participants in postsecondary education had completed less than 9th grade, compared to 18.1 percent of those who did not enroll in a postsecondary program. Also, almost half of all postsecondary

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[10] Participation data were obtained from sample members' survey responses.

Table 6.1

For Sample Members Without a High School Diploma or GED at Random Assignment Who Participated in Basic Education During the Follow-Up Period: Demographic Characteristics and Participation Patterns, by Participation in Postsecondary Education or Training

Characteristic/Outcome	In Postsecondary Education or Training	Not in Postsecondary Education or Training	p-Value
Participation in Basic Education			
Average number of months in basic education	5.8	6.6 *	0.053
Number of months in basic education (%)			
1-6	69.9	64.2	0.105
7-12	21.8	21.7	0.951
13 or more	8.3	14.2 **	0.018
Received a GED during the follow-up period (%)[a]	53.2	12.7 ***	0.000
Demographic Characteristics			
Average age (years)	28.5	30.4 ***	0.000
Any child under 6 (%)	64.8	59.2	0.118
Average highest grade completed	10.1	9.6 ***	0.000
Highest grade completed			
7 or below	2.4	9.5 ***	0.001
8	3.3	8.6 **	0.010
9	15.7	16.1	0.885
10	29.3	30.3	0.779
11	49.2	35.5 ***	0.000
Never married (%)	53.4	51.8	0.670
Living in public housing (%)	9.9	16.3 **	0.016
Employed in prior year (%)	40.3	32.9 **	0.035
Earnings in prior year ($)	1,176.00	883.00	0.129
Average number of months on AFDC in prior year	7.7	8.4 **	0.050
Baseline TALS reading score	191	180	0.281
Preferred educational activity (%)[b]			
Skills training	44.3	35.0 ***	0.009
Basic education	7.7	11.2	0.124
Job search	15.6	18.1	0.367
Reservation wage ($/hour)[c]	7.04	7.14	0.698

(continued)

Table 6.1 (continued)

Characteristic/Outcome	In Postsecondary Education or Training	Not in Postsecondary Education or Training	p-Value
Reservation wage for a position without health benefits ($/hour)[c]	10.52	9.56 **	0.023
Hourly premium for no health insurance ($)[d]	3.43	2.56 ***	0.003
Sample size	198	1,132	

SOURCES: MDRC calculations from information routinely collected by welfare staff, unemployment insurance (UI) earnings records, AFDC records, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

To compensate for differences in sample members' likelihood of being chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen.

Distributions may not add to 100 percent because of rounding.

A two-tailed t-test was applied to differences between characteristics and outcomes for those in postsecondary education or training and those not in postsecondary education or training. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

TALS = Test of Applied Literacy Skills.

[a] The GED credential is given to those who pass the GED test and is intended to signify knowledge of high school subjects

[b] This measure is based on sample members' responses to three questions from the Private Opinion Survey. Each of the three questions asked sample members which of two types of activities they would prefer to attend. From their responses to these questions, it was possible to rank sample members' preference for training, basic education, and job search.

[c] The "reservation wage" is the wage at which sample members expressed to be willing to work when they enrolled in the study.

[d] The "premium for no health insurance" is the additional amount of hourly earnings sample members expressed they would need to take a position without health benefits as opposed to one offering health benefits.

participants had completed 11th grade when they entered the study, compared to 35.5 percent of those who did not participate in postsecondary education or training.

The table also shows that those who attended postsecondary education and training were less disadvantaged in other ways. In the year preceding their entry into the study, 40.3 percent of those who would go on to postsecondary education or training worked. In the other group, this percentage was 32.9. Sample members who did not go on to postsecondary education or training also received welfare for more months in the year preceding random assignment, and they were significantly more likely to live in public housing. Finally, as might be expected, sample members who entered postsecondary education or training were more likely to have expressed a preference for this activity in the Private Opinion Survey (POS) conducted at baseline. However, 35 percent of those not receiving postsecondary education or training also preferred to participate in training but were unable to follow through on this preference, at least not within the two-year follow-up period.

Last, Table 6.1 shows a measure of sample members' expectations about future jobs and wages. This measure, obtained through the POS, captures the hourly wage at which sample members would be willing to take a job. At approximately $7, this "reservation wage" was similar for both groups. However, those who enrolled in postsecondary education or training expressed a much stronger preference for jobs with health benefits. They claimed not to be willing to take a job without health benefits for less than $10.52 per hour. The comparable reservation wage for those not enrolled in postsecondary services was $9.56. This difference (also shown separately as a "premium" for having no health insurance) was statistically significant. This suggests that those enrolled in postsecondary education or training were not necessarily requiring higher wages but were motivated by a quest for jobs with better fringe benefits.

In Table 6.2 we approach the distinction between those who participated in postsecondary education and those who did not in a different way. Using multiple regression analysis, we set out to establish which factors best predicted whether or not adult education participants would go on to receive postsecondary education or training. The table presents the results from this analysis in two ways, depending on available data. The first column shows regression coefficients for all sample members, across the seven sites in the NEWWS study. The second column is limited to sites where a baseline reading test and a Private Opinion Survey (POS) were administered at program intake. This excludes Detroit, Columbus, and Oklahoma City.

The results from these analyses are interesting. Both sets of columns confirm that shorter spells of participation in adult education were associated with a greater likelihood of postsecondary education or training. (The "13 or more" category was left out of the regression model, and the variables flagging spells of 1-6 and 7-12 months were each associated with an increased probability of postsecondary participation, compared to the left-out reference category of 13 or more.) Regardless of the inclusion of test and POS variables, multivariate analyses also supported the importance of GED receipt as an entryway into postsecondary activities. Holding constant all other variables, receipt of a GED was predicted to increase participation in postsecondary education or training by 27.7 percentage points (30.9 percentage points when test scores and POS variables were not included in the regression).

Table 6.2

For Sample Members Without a High School Diploma or GED at Random Assignment Who Participated in Basic Education During the Follow-Up Period: Factors Predicting Participation Patterns, by Test Scores and Preferences

	Without Test Scores and Preferences		Including Test Scores and Preferences	
Explanatory Variable	Estimate	p-Value	Estimate	p-Value
Participation in Basic Education				
Number of months in basic education (%)				
1-6	+7.5 **	0.011	+9.9 ***	0.003
7-12	+6.0 *	0.077	+8.2 **	0.034
Received a GED during the follow-up period (%)[a]	+30.9 ***	0.000	+27.7 ***	0.000
Demographic Characteristics				
Average age (years)	-0.10	0.576	+0.2	0.252
Any child under 6 (%)	-2.9	0.221	-0.7	0.799
Highest grade completed				
7 or below	-24.0 *	0.064	-3.9	0.533
8	-15.9 **	0.037	-7.2	0.131
9	-7.3	0.170	-0.0	0.995
10	-6.5 **	0.044	-4.3	0.116
Never married (%)	-1.8	0.416	-3.5	0.170
Living in public housing (%)	+2.2	0.461	+2.5	0.475
Employed in prior year (%)	+2.6	0.284	+2.5	0.389
Earnings in prior year ($1,000s)	-0.068	0.875	-0.599	0.218
Number of months on AFDC in prior year	-0.3	0.276	-0.3	0.374
Baseline reading score (TALS 150 pts.)			+11.5 ***	0.006
Preferred educational activity (%)[b]				
Skills training			-0.4	0.900
Basic education			+2.6	0.524
Job search			+0.5	0.896
Site				
Atlanta	-5.9	0.416	-6.9	0.369
Grand Rapids	-2.2	0.714	-3.9	0.542
Portland	+0.7	0.904	-11.5	0.166
Columbus	-11.1	0.217		
Detroit	-4.2	0.609		
Oklahoma	+4.4	0.612		

(continued)

Table 6.2 (continued)

	Without Test Scores and Preferences		Including Test Scores and Preferences	
Explanatory Variable	Estimate	p-Value	Estimate	p-Value
Program				
Atlanta HCD	-2.2	0.727	-1.7	0.780
Atlanta LFA	+3.2	0.646	+2.5	0.712
Grand Rapids HCD	+6.0	0.276	+6.6	0.227
Grand Rapids LFA	+9.0	0.166	+9.0	0.156
Portland	+3.3	0.499	+15.5 *	0.051
Riverside HCD	-7.8 *	0.099	-9.9 *	0.063
Riverside LFA	+10.6 *	0.089	+12.6 *	0.097
Columbus Integrated	+1.8	0.845		
Columbus Traditional	+11.3	0.237		
Detroit	+9.9	0.324		
Oklahoma	-0.3	0.973		
R^2	0.164		0.188	
Sample size	1,278		887	

SOURCES: MDRC calculations from information routinely collected by welfare staff, unemployment insurance (UI) earnings records, AFDC records, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

To compensate for differences in sample members' likelihood of being chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen.

Results presented in this table were obtained from ordinary least squares regression analyses, controlling for baseline characteristics, research group (program or control group), participation in adult education, GED receipt, and barriers and attitudes measured with the Private Opinion Survey.

Distributions may not add to 100 percent because of rounding.

A two-tailed t-test was applied to differences between characteristics and outcomes for those in postsecondary education or training and those not in postsecondary education or training. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

TALS = Test of Applied Literacy Skills.

HCD = Human Capital Development.

LFA = Labor Force Attachment.

[a] The GED credential is given to those who pass the GED test and is intended to signify knowledge of high school subjects.

[b] This measure is based on sample members' responses to three questions from the Private Opinion Survey. Each of the three questions asked sample members which of two types of activities they would prefer to attend. From their responses to these questions, it was possible to rank sample members' preference for training, basic education, and job search.

Some of the estimates varied with the availability of test score and POS preference data for the multivariate analysis. In the leftmost column of Table 6.2, which was estimated without these data, there are substantial effects associated with different grade levels at program entry. Compared to those who completed 11th grade (the left-out category), 8th grade completers were 15.9 percent less likely to enter postsecondary education programs. Those who had not yet completed 8th grade when they entered the study were 24.0 percent less likely to participate in postsecondary programs. All this is unsurprising, because even two years of adult education may not be sufficient to provide sample members entering at 8th grade with the skills to gain access to postsecondary education and training services. However, when baseline test scores and preferences are included in the analysis (as shown in the third and fourth columns of Table 6.2), baseline test scores appear to be more important predictors of postsecondary participation than completed grade levels. (The latter no longer were statistically significant predictors.)[11] This effect was independent of sample members' receipt of a GED credential. Other demographic variables, site variables, and even program variables generally were not statistically significant predictors of postsecondary participation. Therefore, within the limitations of our nonexperimental methods, our data support the notion that boosting basic skills levels and GED receipt are the most promising ways to increase access to postsecondary services among welfare recipients in adult education programs (provided that postsecondary services are available in the program context they face). In this regard, lack of time in these programs to accomplish such improvements would be an important barrier to gaining access to postsecondary services.

C. Patterns of Participation in Postsecondary Activities

Enrollment in postsecondary education and training was not concentrated in specific parts of the two-year follow-up period. As Figure 6.1 illustrates, the percentage of adult education participants who also received postsecondary services increased slowly from less than 2 percent in month 1 to a high of more than 5 percent in month 16. By the end of follow-up, more than 4 percent of sample members who had received adult education were still in skills training or college, and it is possible that more students enrolled in postsecondary activities after the survey follow-up ended. This overall pattern of participation confirms that welfare recipients in need of basic education need time to reach postsecondary activities and need time to complete those activities. Table 6.3 shows that, of all participants who enrolled in skills training or college during the follow-up period (the second column), 28.8 percent were still enrolled at the end of follow-up. Also, the table shows that almost a year passed before the average participant enrolled in skills training or college. This reflects the fact that, in many cases, postsecondary participants spent substantial time in adult education programs during the first half of the two-year follow-up period.

Figure 6.2 displays these participation patterns in a different way. Including only those who ever enrolled in postsecondary services, it shows cumulative rates of enrollment in and completion or termination of those services.[12] The solid line indicates that more than 35 percent

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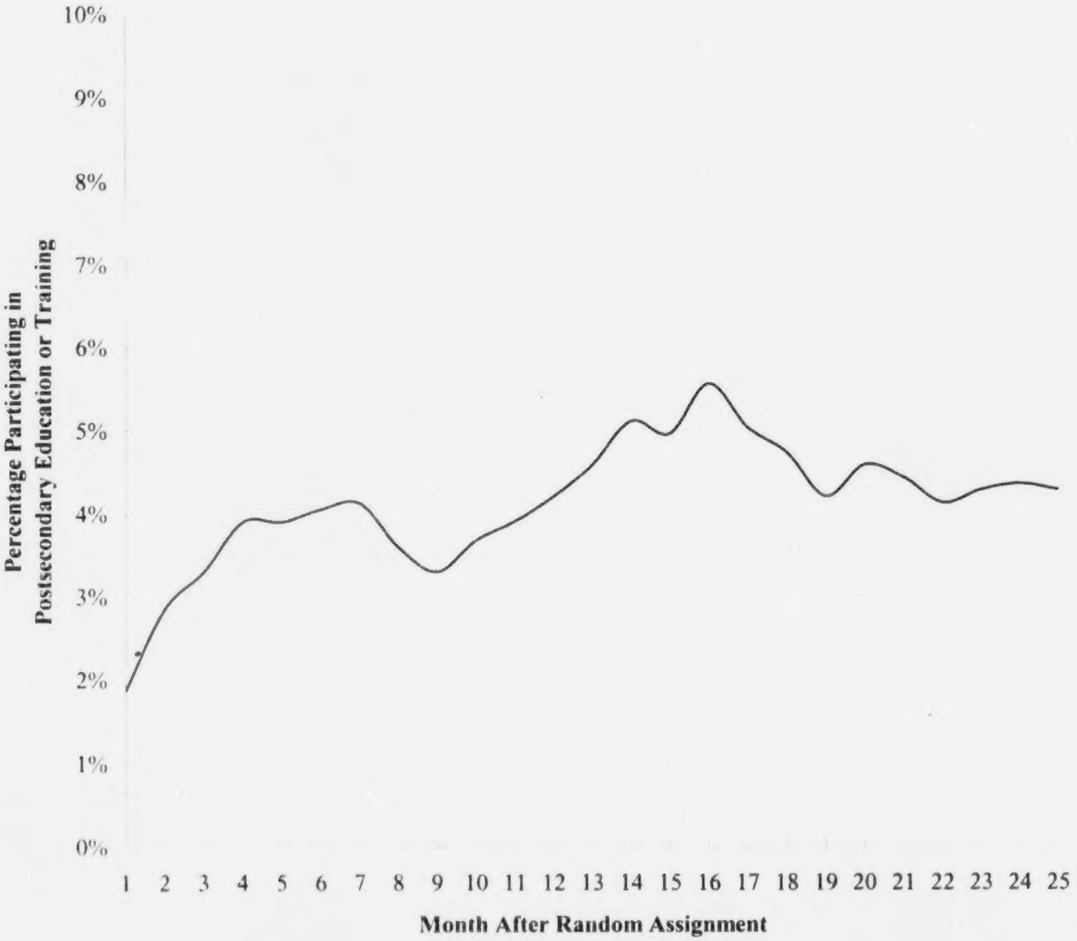
[11] Using the "tolerance" option in the statistical program, we checked whether these changes in the relative importance of the grade variables arose because the regression model had too many highly correlated independent variables (a problem known as "multi-colinearity"). This was not the case. Even with the baseline test scores in the model, the grade variables contributed a meaningful amount of independent explanatory power to the model.

[12] It is not possible to distinguish between successful completions and unsuccessful terminations, because not all postsecondary training programs end with a specific credential, as discussed below. Also, even training or college attendance that is interrupted prematurely may increase participants' skills and benefit their subsequent employment outcomes; it could therefore be deemed "successful."

Figure 6.1

For Sample Members Without a High School Diploma or GED at Random Assignment Who Participated in Basic Education During the Follow-Up Period: Monthly Rate of Participation in Postsecondary Education or Training



SOURCE: MDRC calculations from the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

To compensate for differences in sample members' likelihood of being chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen.

Table 6.3

For Sample Members Without a High School Diploma or GED at Random Assignment Who Participated in Basic Education During the Follow-Up Period: Participation Patterns by Participation in Postsecondary Education or Training

Outcome	All Participants in Basic Education	In Postsecondary Education or Training
Ever participated in postsecondary education or training (%)	14.9	100.0
Number of months in postsecondary education or training (%)		
1-3	3.5	23.2
4-6	5.0	33.8
7-12	4.3	28.8
13 or more	2.1	14.1
Average number of months participated	1.0	6.9
Average first month of participation	N/a	10.6
Still participating at end of follow-up period (%)	4.3	28.8
Average month of completion for those who completed	N/a	13.9
Received a trade license or training certificate (%)	6.6	33.8
Sample size	1,330	198

SOURCE: MDRC calculations from the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

To compensate for differences in sample members' likelihood of being chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen.

N/a = not available or applicable.

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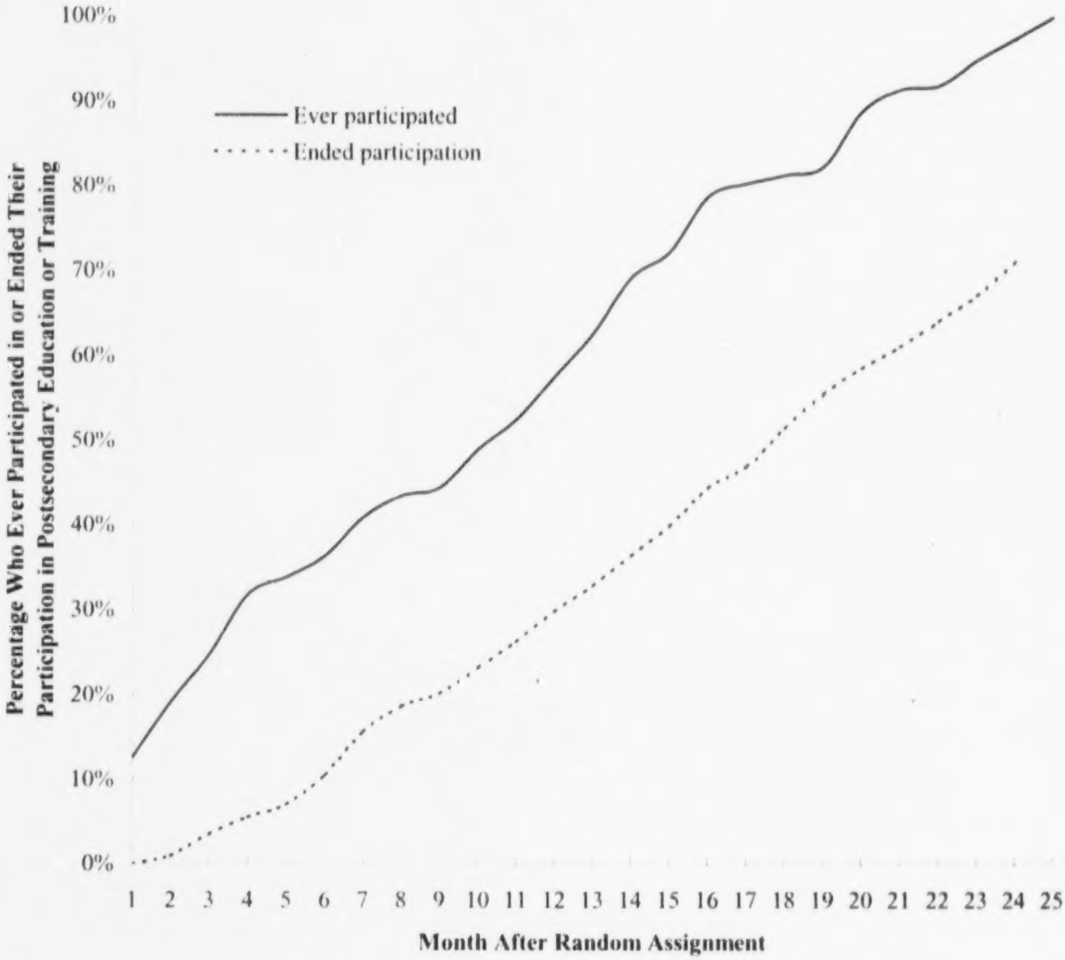
Figure 6.2

For Sample Members Without a High School Diploma or GED at Random Assignment Who Participated in Basic Education During the Follow-Up Period: Cumulative Rates of Participation in and Completion or Termination of Postsecondary Education or Training



SOURCE: MDRC calculations from the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

To compensate for differences in sample members' likelihood of being chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen.

of eventual participants had enrolled by month 6, while more than half did so by month 11. But one out of five participants did not enroll until month 17, almost a year and a half after their random assignment in the NEWWS Evaluation. Completion and termination rates show a similar pattern, with a delay of about six months. About half of all participants ended their participation in skills training or college by month 18, with less than 30 percent doing so within the first year of follow-up. As indicated before, only 71.2 percent of all participants ended their participation in skills training or college programs during the two-year follow-up period. At follow-up, 28.8 percent were still enrolled.

The low overall levels of enrollment in postsecondary programs resulted in adult education students adding only about a month of postsecondary services to their total education experience, on average. For those who did receive postsecondary services, the average was about 6.9 months, as shown in Table 6.3, although this number is truncated because many participants were still enrolled at the time of the follow-up interview. More than a third of participants in skills training or college spent between four and six months in such programs, with 42.9 percent spending more than six months and 14.1 percent spending more than a year. About a quarter (23.2 percent) of postsecondary participants spent fewer than four months in skills training or college.

Last, Table 6.3 measures attainment of a trade license or training certificate. Successful spells of participation in postsecondary programs were likely completed with such a credential, although some training programs do not award formal credentials, and in other cases students may find employment before they can formally graduate from a training program. About a third of participants in skills training or college reported receiving a training credential, which, given that 28.8 percent were still enrolled at follow-up, means that more than a third of enrollees in such programs ended them without a credential. In a later section we assess the importance of a training credential over and above the participation in skills training or college itself.

IV. Effects of Postsecondary Services on Earnings and Self-Sufficiency

A. Analytical Approach

To capture the effects of postsecondary education and training on subsequent earnings and welfare receipt, we examined these outcomes relative to each sample member's completion of her last spell of adult education. The end of participation in adult education was used as the "baseline" in our analysis so that all participants would be at a comparable point in their individual program experience, regardless of how much time they spent in adult education. Also, such a post-adult education baseline sets up a distinction between two different trajectories of subsequent activities. The first trajectory describes the experiences of welfare recipients who continued their education and training, enrolling in postsecondary programs. The other trajectory follows a larger group of sample members for whom adult education was their last education activity within the two-year follow-up.

The fact that the survey follow-up was limited to two years has consequences for the reliability of these analyses. Many sample members for whom we analyze earnings and welfare outcomes in years beyond the first two years of the study may have participated in additional adult education or in postsecondary skills training or college programs after this two-year follow-up period ended. To the extent that such subsequent participation affected the outcomes we analyze, our estimates will be inaccurate. Fortunately, future analytical work can assess the extent of these inaccuracies after data become available from a five-year survey currently being conducted.

In the meantime, we sought to limit the consequences of unmeasured participation in education or training by excluding from our analyses all sample members who were still enrolled in education or training at the time of the two-year follow-up survey. If they were enrolled in adult education, we would not have been able to establish the baseline for our analysis for these sample members. If they were enrolled in skills training or college, we would not have captured the full extent of their participation in those services. Thus, subsequent results represent 1,082 sample members (program and control group members) who (1) had no high school diploma or GED when they entered the NEWWS Evaluation, (2) participated in adult basic education or GED preparation during the follow-up period, (3) completed that participation before the end of follow-up, and (4) were not currently enrolled in skills training or college at the end of follow-up. For these sample members we analyzed up to three years (12 quarters) of post-adult education earnings and welfare receipt.[13]

B. Effects on Earnings and Welfare Receipt

Figure 6.3 shows how quarterly earnings varied with participation in skills training or college.[14] The two lines shown in this figure represent regression-adjusted estimates of earnings relative to the month in which sample members ended their last spell of adult education.[15] Regression adjustment uses data on a range of baseline variables (including variables measuring employment history, education, welfare receipt, and demographic characteristics) to eliminate the effect of these variables on apparent differences between participants in skills training or college and nonparticipants. Thus, the lines in Figure 6.3 compare these two groups as though they were identical in these characteristics and varied only in their exposure to skills training or college. The vertical line in the figure indicates the first month following the last spell of adult education. Thus, participants were in adult education to the left side of that vertical line, as evidenced by the low level of earnings during those quarters. After completing their last spell of adult education, sample members rapidly increased their employment and earnings, regardless of whether they would enroll in skills training or not. The dashed line shows earnings for sample members who did not receive skills training, while the solid line shows earnings for sample members who did. During the first two post-adult education years (eight quarters), earnings for the two groups differed little. However, starting in the ninth quarter, significant and persistent differences in favor of those who received skills training began to appear. In the 11th quarter, the estimated difference reached

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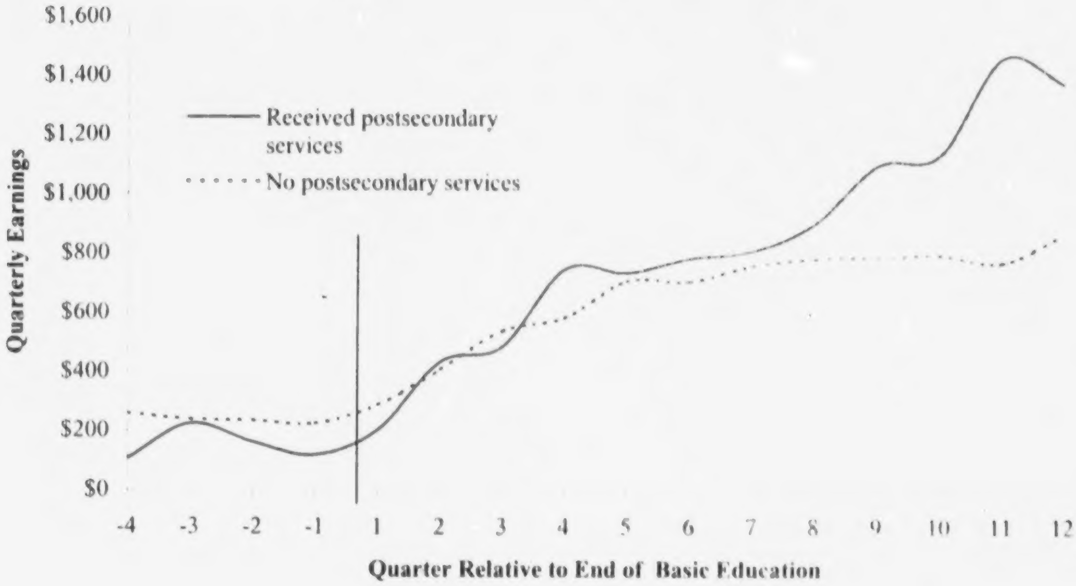
[13] The length of available post-basic education follow-up is affected by the timing of sample members' entry into the NEWWS Evaluation and the timing of their completion of participation in adult education. Those who were randomly assigned earlier or completed their adult education earlier have more quarters of follow-up data than those who completed or were assigned later. To avoid biases associated with these timing issues, we control for the month of adult education completion and the relative month of random assignment in our analyses (in addition to 24 other baseline variables). Sample sizes for individual quarters following completion of adult education were as follows: Q1-3: 1,082; Q4: 1,080; Q5: 1,074; Q6: 1,069; Q7: 1,058; Q8: 1,027; Q9: 921; Q10: 831; Q11: 722; and Q12: 604.

[14] As in other chapters in this report, earnings and employment measures were obtained from unemployment insurance earnings records.

[15] Regression adjustment was done using ordinary least squares analyses. A complete list of control variables is included in the box on page 6-10.

Figure 6.3

For Sample Members Without a High School Diploma or GED at Random Assignment Who Participated in Basic Education During the Follow-Up Period: Earnings Relative to the End of Their Participation in Basic Education, by Participation in Postsecondary Education or Training



SOURCES: MDRC calculations from information routinely collected by welfare staff, unemployment insurance (UI) earnings records, AFDC records, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

To compensate for differences in sample members' likelihood of being chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen.

Results presented in this table were obtained from ordinary least squares regression analyses, controlling for baseline characteristics, research group (program or control group), participation in adult education, GED receipt, and barriers and attitudes measured with the Private Opinion Survey.

$693. This represents an increase of 90 percent relative to the earnings of those who did not enroll in postsecondary services.

Because of the nonexperimental nature of this analysis, it is not possible to ascribe this difference to receipt of postsecondary education and training with certainty. It is possible that welfare recipients who enrolled in postsecondary programs were different from welfare recipients who did not enroll in those programs in ways that could not be controlled in the analysis. However, all estimates were adjusted for important predictors of participation in skills training or college, such as GED receipt, baseline literacy scores, NEWWS program assignment, and site. Also, it is encouraging that the differences in earnings between the two groups did not begin to appear until well after both adult education participation and postsecondary participation had ended. Surprisingly, at the end of the last adult education spell, and during the first two years afterward, there was no evidence of any significant differences between postsecondary participants and those not receiving postsecondary services. One might have expected some negative differences, inasmuch as those in skills training or college were supposedly less available for work.

Standard Control Variables

Analyses featured in this chapter always use the same set of basic control variables. The purpose of these variables is to isolate the effects of postsecondary education or training even if those are correlated with other variables, such as demographics, motivation, or welfare history. The list of standard control variables follows:

Site (city, state)
Assignment to Job Opportunities and Basic Skills Training (JOBS) program
Marital status
Number of children
Any child older than 5
Ethnic group
Age
Gender
Employed in prior year/prior quarter
Earnings in prior year/prior quarter
Received AFDC/Food Stamps in prior year/prior quarter
Number of months received AFDC/Food Stamps in prior year
Average amount of AFDC/Food Stamps per month in prior year
Test of Applied Literacy Skills (TALS) reading test score
Receipt of a GED or high school diploma during the follow-up period
Relative month of random assignment
Available post-basic education follow-up

According to Figure 6.3, it takes a substantial amount of time for payoffs to participation in skills training or college to manifest themselves. As pointed out earlier in this chapter, the average length of participation in postsecondary services was about seven months for those who participated (possibly somewhat longer when truncation resulting from the limited survey follow-up is considered). During that time, we would not expect to see earnings gains associated with postsecondary participation, because sample members were kept out of the labor market—an effect representing the opportunity cost of participation. However, substantial differences in earnings did not appear until the ninth quarter, or almost a year and a half later. This suggests that the added skills acquired through postsecondary participation are not immediately valued in the labor market, either because those leaving postsecondary classes do not find work immediately or because they start out in entry-level jobs. Only after some time did they appear to be "rewarded" with more steady employment, possibly more responsibilities on the job, and greater earnings growth. Typical studies of welfare or job training programs might not find such delayed effects, because (1) such studies usually do not use the end of an education spell as their starting point, and (2) most studies do not have a long enough follow-up period to find long-term effects.

In Figure 6.4, we compare welfare receipt across the two groups of adult education participants. The solid line shows the quarterly amount of welfare received by those who participated in postsecondary education or training, and the dashed line shows the same outcome for those who did not receive postsecondary services during the two-year follow-up period. As might be expected, welfare receipt peaked around the time that sample members completed their last spell of adult education and declined quickly afterward. Through the second post-education year, this decline was comparable for both groups, after which it leveled off for those who did not participate in postsecondary programs and accelerated for those who did. During each of the last four quarters shown in the graph, the difference in welfare receipt between the two groups was statistically significant, amounting to a reduction of close to 50 percent in the final quarter. Again, the figure suggests that participation in postsecondary education or training may have substantial effects, which, however, occur with considerable delay and therefore are easily missed.

Last, in Table 6.4 we show year-by-year effects on earnings and the amount of welfare received as well as effects on rates of employment and welfare receipt. Across all outcomes, the table shows how differences between those who received postsecondary services and those who did not were concentrated in the third year after adult education ended. Earnings during that year were $1,542, or 47.3 percent higher for postsecondary participants, and the amount of AFDC received was $919, or 32.1 percent lower. Quarters of employment went up by 0.4, or 24.8 percent during year 3. The fact that the earnings difference was almost twice as large as that (in relative terms) suggests that sample members who received postsecondary services not only were more likely to work but also had higher earnings when they did. Again, it is possible that some of this apparent effect reflects uncontrolled differences between those who received postsecondary services and those who did not. However, given that all sample members featured in this table received adult education services and that their initial skill levels, employment histories, and demographic characteristics were controlled, these estimates offer some support for the pursuit of postsecondary services by welfare recipients after they leave adult education programs.

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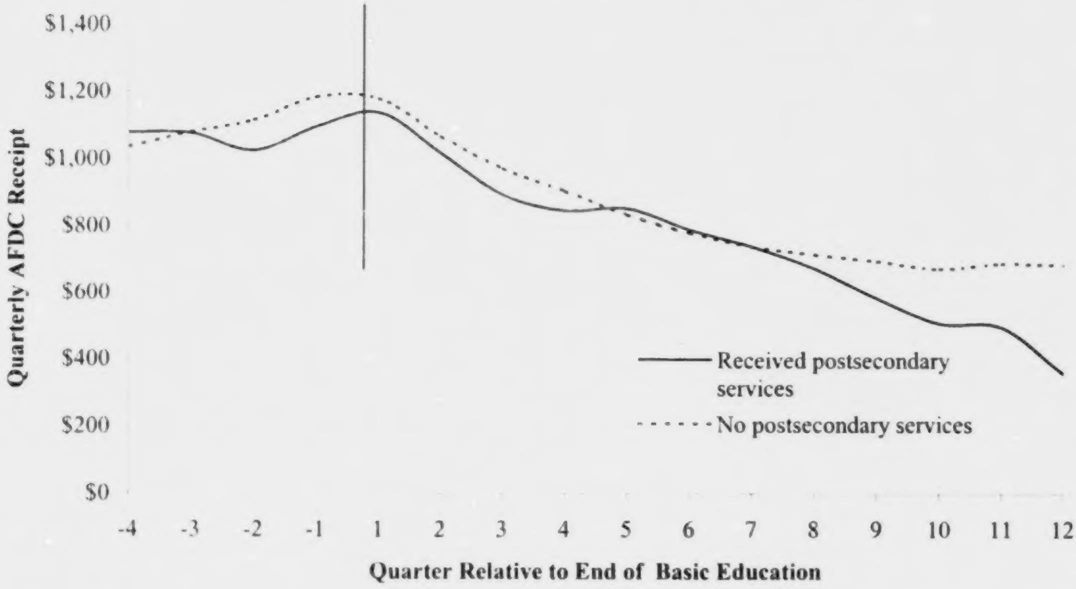
Figure 6.4

For Sample Members Without a High School Diploma or GED at Random Assignment Who Participated in Basic Education During the Follow-Up Period: AFDC Received Relative to the End of Their Participation in Basic Education, by Participation in Postsecondary Education or Training



SOURCES: MDRC calculations from information routinely collected by welfare staff, unemployment insurance (UI) earnings records, AFDC records, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

To compensate for differences in sample members' likelihood of being chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen.

Results presented in this table were obtained from ordinary least squares regression analyses, controlling for baseline characteristics, research group (program or control group), participation in adult education, GED receipt, and barriers and attitudes measured with the Private Opinion Survey.

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Table 6.4

For Sample Members Without a High School Diploma or GED at Random Assignment Who Participated in Basic Education During the Follow-Up Period: Estimated Effects of Participation in Postsecondary Education or Training on Earnings, Employment, and AFDC Receipt

Outcome	Sample Size	In Post-Secondary Education or Training	Not in Post-Secondary Education or Training	Difference (Impact)	Percentage Change[a] (%)	p-Value
Outcomes Relative to Quarter in Which Basic Education Ended						
Total earnings ($)						
Year 1	1,080	1,871	1,820	51	2.8	0.871
Year 2	1,027	3,262	3,012	250	8.3	0.544
Year 3	604	4,802	3,261	1,542 **	47.3	0.013
Ever employed (%)						
Year 1	1,080	53.4	49.4	4.1	8.3	0.369
Year 2	1,027	61.9	56.0	5.9	10.5	0.212
Year 3	604	70.4	54.4	16.0 ***	29.5	0.006
Number of quarters employed						
Year 1	1,080	1.2	1.2	0.0	1.9	0.862
Year 2	1,027	1.7	1.6	0.1	8.7	0.379
Year 3	604	2.0	1.6	0.4 **	24.8	0.048
Total AFDC ($)						
Year 1	1,082	3,895	4,124	-229	-5.5	0.216
Year 2	1,033	3,048	3,091	-43	-1.4	0.854
Year 3	607	1,942	2,862	-919 ***	-32.1	0.003
Ever received AFDC (%)						
Year 1	1,082	88.7	92.7	-3.9	-4.3	0.101
Year 2	1,033	77.0	75.4	1.6	2.1	0.680
Year 3	607	53.7	68.3	-14.6 ***	-21.4	0.007
Quarters receiving AFDC						
Year 1	1,082	3.3	3.3	-0.1	-2.4	0.477
Year 2	1,033	2.8	2.6	0.2	5.8	0.328
Year 3	607	1.8	2.4	-0.6 ***	-24.5	0.006

(continued)

Table 6.4 (continued)

SOURCES: MDRC calculations from information routinely collected by welfare staff, unemployment insurance (UI) earnings records, AFDC records, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

To compensate for differences in sample members' likelihood of being chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen.

Results presented in this table were obtained from ordinary least squares regression analyses, controlling for baseline characteristics, research group (program or control group), participation in adult education, GED receipt, and barriers and attitudes measured with the Private Opinion Survey.

A two-tailed t-test was applied to differences between outcomes for those in postsecondary education or training and those not in postsecondary education or training. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

[a] "Percentage change" equals 100 times the "difference" (impact) between the two comparison groups divided by the outcome for those not in postsecondary education or training.

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C. Do Training Credentials Matter?

In the present sample, 34.3 percent of postsecondary participants reported receipt of a training credential on the two-year survey. When those still enrolled at that time are excluded, this share increases to 40.1 percent. An important question in judging the effectiveness of postsecondary education and training services is whether these services require a credential to be valuable in the labor market. This question was addressed in a report on the New Chance program for teen mothers, which found that receipt of a credential contributed substantially to the employment and earnings of the young women in that study.[16] In fact, Quint et al. found that, without a credential, enrollment in skills training programs had few benefits.

In this study, we assessed the importance of training credentials by including receipt of such a credential as an additional explanatory variable in the regression models used to produce the estimates in Figure 6.3 and Table 6.4. Using this multivariate method allows the regression analysis to establish which aspect of participation in postsecondary activities mattered most: the actual training received or the credential associated with it. This is similar to analyses presented in Chapter 4, in which we included GED receipt and education participation variables in the same model. (It also is comparable to the approach used by Quint et al.)

Figure 6.5 suggests that most of the effect of postsecondary services in our study arose from the training or college participation itself, not from the credentials that often accompany such participation. The figure represents the regression results, underscoring this point as follows. The dashed "baseline" profile shows quarterly earnings for sample members who did not participate in skills training or college and also did not receive a training credential.[17] Next, the solid line with the "×" marks presents estimated earnings for those who participated in skills training or college but who had not received a training credential by the time of the follow-up interview. Some of these participants might have earned such a credential after the survey, although, as pointed out above, anyone who was in a program at the time of the interview was excluded from these analyses altogether. The third, solid line shows the estimated combined effect of postsecondary participation *and* credential receipt. With relatively few sample members receiving postsecondary services to begin with, the difference between the two solid lines in Figure 6.5 was not estimated precisely and in most quarters was not statistically significant. However, in each of the last four quarters shown in Figure 6.5, the difference in estimated earnings between postsecondary participants and nonparticipants (that is, the gap between the dashed line and the lower solid line with the × marks) remained statistically significant. This suggests that, at least in this sample of welfare recipients, participation in skills training or college may have been more important than the credential that accompanied successful completion.

It is unclear why training credentials appeared to be so much more important in the New Chance demonstration, studied by Quint et al. (1997), than in the analyses presented here. An important difference is that participation in the New Chance program was voluntary, whereas

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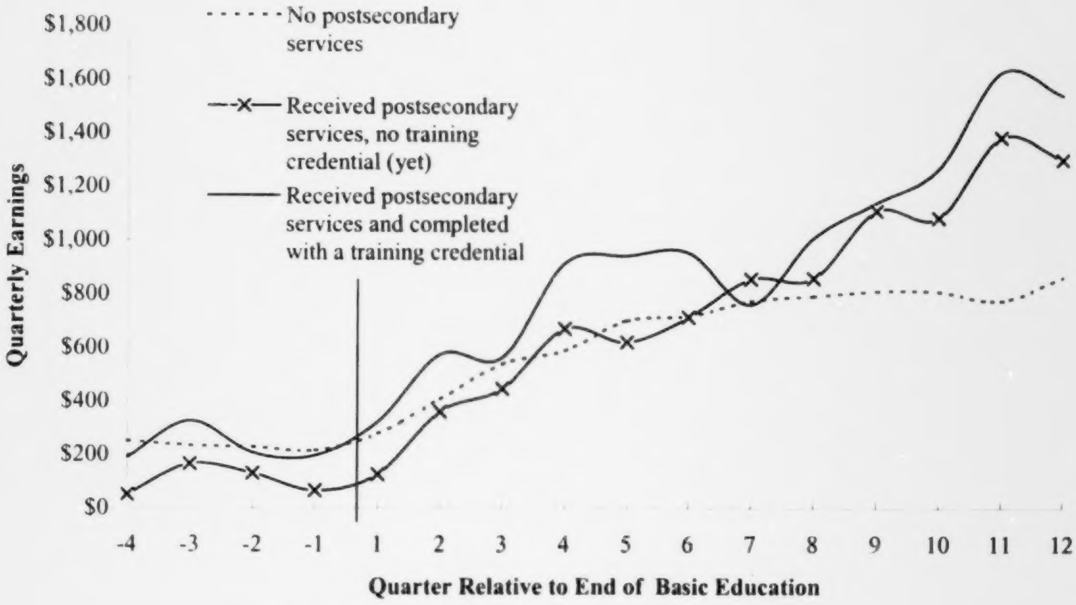
[16] Quint et al., 1997, p. ES14.

[17] This dashed line is somewhat different from the one presented in Figure 6.3. This reflects the fact that some sample members who did not participate in postsecondary activities *did* report having a training credential or certificate. In Figure 6.3, these sample members would have been included in the dashed line. Here, that line was estimated without them.

Figure 6.5

For Sample Members Without a High School Diploma or GED at Random Assignment Who Participated in Basic Education During the Follow-Up Period: Earnings Relative to the End of Their Participation in Basic Education, by Participation in Postsecondary Education or Training and Receipt of a Training Credential



SOURCES: MDRC calculations from information routinely collected by welfare staff, unemployment insurance (UI) earnings records, AFDC records, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

To compensate for differences in sample members' likelihood of being chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen.

Results presented in this table were obtained from ordinary least squares regression analyses, controlling for baseline characteristics, research group (program or control group), participation in adult education, GED receipt, and barriers and attitudes measured with the Private Opinion Survey.

most participation in the present study was mandatory. In a voluntary program, failure to complete skills training with a credential may signal a lack of persistence. In the context of a mandatory welfare-to-work program, such a failure to complete may signal more positive events, such as the sample member's getting a job or leaving welfare. However, the effects of a credential also may depend on other factors, such as the health of the labor market (better now than during the New Chance years), the average age of sample members (higher in the present study), or the type of skills being acquired (a substantial proportion of New Chance trainees received training in nursing, which requires certification to be useful in the labor market).

D. Effects of Variation in the Duration of Postsecondary Activities

In addition to estimating effects of postsecondary participation per se, it might be useful to know the *marginal* effect of additional months of participation, similar to the analyses surrounding adult education in Chapter 3 of this report. However, for several reasons, such an analysis is difficult. First, we do not know the intended duration of the training programs in which participants were enrolled. The length of training classes varies with the type of skill learned and with the structure of the training program. Certain skills may be taught and mastered in a matter of weeks or months, whereas others require years of study and practice. Because the two-year survey did not record the type of skill for which sample members were trained, it is impossible to approximate an "intended duration" without knowing more about the actual programs. This in turn means that a program duration which is ideal in one situation may, in another situation, signal a dropout or a student who is languishing. We cannot distinguish among these situations. Also, a substantial number of participation spells were truncated at the end of the two-year survey follow-up period, making estimates of the effects of additional participation unreliable. And last, the interpretation of effects of additional participation is difficult in an analytical framework that does not link outcomes in specific months to participation in the same or preceding months. In our analyses, which, given the limited survey follow-up, do not allow for month-by-month matching of participation and outcomes, higher participation could mean fewer opportunities to work and lower earnings in one month but more accumulated training and higher earnings in another month. Estimated effects of additional participation therefore combine opportunity costs in certain months with payoffs in other months. The timing of these effects varies across individual sample members.

With these caveats in mind, we can turn to Table 6.5, which shows estimated effects of an additional month of participation in postsecondary activities on earnings and welfare received during the first, second, and third year following the end of adult education. As might be expected, during the first two post-adult education years, estimated effects of participation in skills training or college were actually negative, albeit modest. This represents the opportunity cost of participating, in terms of lost employment opportunities and earnings. In the third year, the estimated effect of additional participation became positive, although it was not statistically significant. However, the reduction in welfare that accompanied this earnings effect was statistically significant (as shown in the table's bottom panel). Again, it must be emphasized that these estimated effects, both positive and negative, are likely smaller than the actual costs and benefits of participation to individual participants, since they combine estimates of current costs for ongoing participants with estimates of longer-term benefits for those who completed a postsecondary program.

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Table 6.5

For Sample Members Without a High School Diploma or GED at Random Assignment Who Participated in Basic Education During the Follow-Up Period: Effects of an Additional Month of Participation in Postsecondary Education or Training on Earnings and AFDC Receipt

	Estimated Effect	p-Value
	Outcomes Relative to Quarter in Which Basic Education Ended	
Total earnings ($)		
Year 1	-82 **	0.043
Year 2	-68	0.199
Year 3	115	0.142
Total AFDC ($)		
Year 1	-32	0.176
Year 2	-20	0.505
Year 3	-123 ***	0.002

SOURCES: MDRC calculations from information routinely collected by welfare staff, unemployment insurance (UI) earnings records, AFDC records, Private Opinion Survey data, and the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

To compensate for differences in sample members' likelihood of being chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen.

Results presented in this table were obtained from ordinary least squares regression analyses, controlling for baseline characteristics, research group (program or control group), participation in adult education, GED receipt, and barriers and attitudes measured with the Private Opinion Survey.

A two-tailed t-test was applied to estimated regression coefficients. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

V. Conclusion and Implications

Within the limitations of the nonexperimental analyses from which they emerged, the findings presented in this chapter support continued participation by welfare recipients in skills training or college programs after they complete adult education. Although improvements in earnings and self-sufficiency usually follow spells of adult education, longer-term improvements in these outcomes were substantially stronger for those who also participated in skills training or college. At the same time, the opportunity costs of participating in postsecondary programs appear limited, inasmuch as these programs tend to be short (about seven months, on average). There is evidence that shorter programs have somewhat lower opportunity costs (as expected), while the limited available evidence suggests that the benefits of *extended* participation in postsecondary programs appear limited. A training credential may help to enhance the value of postsecondary services, but our analyses suggest that participants may have benefited even if they did not receive such a credential.

Given the positive estimated effects of postsecondary services on welfare recipients' outcomes, it is problematic that so few participants in adult education programs receive such postsecondary services. In this sample, only 14.9 percent of those who enrolled in adult education also received postsecondary services during a two-year follow-up period. Maybe this figure is low because of the limited time during which we observed education outcomes in this sample. However, it is likely that many sample members sought employment while they were still in adult education or almost immediately afterward. In a welfare-to-work environment characterized by "work first" programs and quick transitions off of welfare, the longer-term education and training needs of welfare recipients with low basic skills may be pushed to the background. In the short term, the consequences of such an approach are not obvious. However, this chapter suggests that these consequences could be significant in the long run.

Appendix A

Data Sources and Research Samples

This report uses a wide array of data sources and research samples in its analyses. Our general strategy in choosing research samples was to use the largest sample appropriate for each subject. Thus, for example, an analysis looking at impacts on three years of earnings that does not require the use of survey data would use the sample shown in box 3 of Figure A.1. The figure depicts the relationships among the major samples used in this report; the sample sizes shown in Figure A.1 and used throughout the majority of this report include *only sample members without a high school diploma or GED at random assignment.*[1]

This appendix first describes each of the data sources used in this report and then outlines its research samples.

I. Data Sources

Baseline Characteristics Data. Just before random assignment, data were collected by welfare staff on the characteristics of each research sample member, including basic demographic information. Baseline data are available for all sample members.

Also before random assignment, most sample members in four of the seven NEWWS Evaluation sites (Atlanta, Grand Rapids, Riverside, and Portland) completed a confidential Private Opinion Survey (POS). This brief survey asked respondents about their attitudes, opinions, and preferences regarding work and welfare. POS data are available for the majority of the full impact sample in Grand Rapids and Portland, but both Riverside and Atlanta had an early cohort for whom data are not available.[2]

In addition, Atlanta, Grand Rapids, and Riverside administered reading and math tests. In Atlanta and Grand Rapids, clients were administered the Test of Applied Literacy Skills (TALS) document literacy test and the Comprehensive Adult Student Assessment System (CASAS) math test. In Riverside, CASAS tests were used for both reading and math.[3] Riverside's CASAS reading scores were converted to TALS document literacy test scores using a "crosswalk" algorithm developed by researchers at Boston College.[4]

Two-Year Client Survey Data. Much of the follow-up data used in our analyses are based on results compiled from a survey administered approximately two years after random as-

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[1] About one-quarter of program group members in Riverside's education-focused program had a high school diploma or GED at random assignment but scored low on either the math or the reading portion of the appraisal test, or required limited English, and thus were determined to be in need of basic education. These sample members are included in the samples used in the majority of this report. Thus, when this report refers to "sample members without a high school diploma or GED at random assignment" it refers to sample members without a high school diploma or GED at random assignment in all programs but Riverside's education-focused program. In Riverside's education-focused program the heading refers to both individuals without a high school diploma or GED at random assignment or those with low test scores or limited English skills. The exception to this rule is Chapter 4, which excludes from its analysis individuals in Riverside's education-focused program with a high school diploma or GED at random assignment.

[2] In Riverside, data are not available for those randomly assigned from September 1991 to mid-February 1992; in Atlanta, data are not available for those sample members randomly assigned from January to mid-March 1992.

[3] The specific versions of the CASAS offered were the GAIN Appraisal Reading and Math tests.

[4] Haney et al., 1996.

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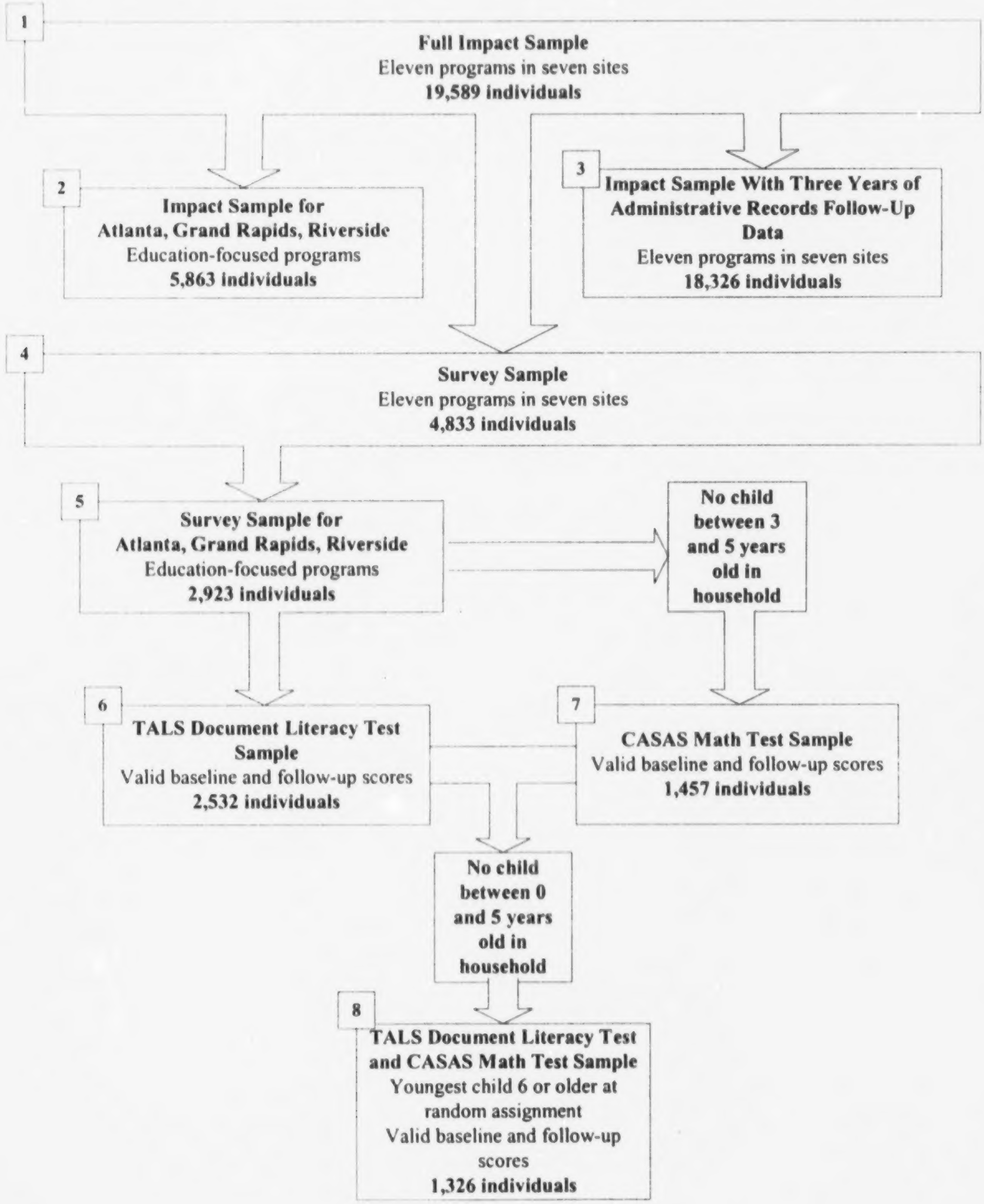
Figure A.1

Samples and Subsamples Used in This Report: Sample Members Without a High School Diploma or GED at Random Assignment


1
Full Impact Sample
Eleven programs in seven sites
19,589 individuals
2
Impact Sample for Atlanta, Grand Rapids, Riverside
Education-focused programs
5,863 individuals
3
Impact Sample With Three Years of Administrative Records Follow-Up Data
Eleven programs in seven sites
18,326 individuals
4
Survey Sample
Eleven programs in seven sites
4,833 individuals
5
Survey Sample for Atlanta, Grand Rapids, Riverside
Education-focused programs
2,923 individuals
No child between 3 and 5 years old in household
6
TALS Document Literacy Test Sample
Valid baseline and follow-up scores
2,532 individuals
7
CASAS Math Test Sample
Valid baseline and follow-up scores
1,457 individuals
No child between 0 and 5 years old in household
8
TALS Document Literacy Test and CASAS Math Test Sample
Youngest child 6 or older at random assignment
Valid baseline and follow-up scores
1,326 individuals

signment to a subset of sample members in each of the sites in the evaluation.[5] Survey respondents were asked about a wide range of topics including their participation in education and training activities, whether they received a GED or high school diploma since entering the study, their opinions about work and welfare, and their employment history. Also, as part of the two-year follow-up survey, all program and control group members in Atlanta, Grand Rapids, and Riverside were administered the TALS document literacy test. Individuals in these sites were also given the CASAS math test if they did not have a child age 3 through 5 at random assignment.[6]

Administrative Records Data. Earnings and public assistance impacts were computed using unemployment insurance (UI) records and AFDC administrative records. Two years of UI records and AFDC administrative records are available for all sample members in all seven sites. Three years of UI records and AFDC administrative records are available for all sample members except for a late cohort in Oklahoma City.[7] Sixty-seven percent of the full impact sample in Oklahoma has three years of administrative records data.

Adult Education Teacher Survey and Administrator Interviews. Adult education teachers were surveyed in Atlanta, Grand Rapids, and Riverside during the fall and winter of 1993. MDRC researchers determined which education providers to survey by working with local welfare office staff to identify the major agencies offering education services to welfare recipients. The teachers in those agencies were asked to describe their program and to rate it in terms of such issues as linkages with welfare-to-work staff, instructional styles, measures of student progress, and class size. A total of 24 teachers in Atlanta, 79 teachers in Grand Rapids, and 45 teachers in Riverside—representing 33 institutions—are included in the results presented in this report. In addition, during a visit to each of the adult education institutions included in the teacher survey, an in-person interview was conducted with the program's administrator. The Adult Education Teacher Survey is this report's main source of data on the characteristics of the adult education providers.

II. Research Samples

Impact Sample. The sample of individuals for whom administrative records and basic demographic baseline data are available is known as the *impact sample*. MDRC has collected two-years of follow-up administrative records data for the full impact sample in all 11 programs in the NEWWS Evaluation. Box 1 of Figure A.1 shows that these data are available for 19,589 sample members without a high school diploma or GED at random assignment.

Two subsets of the full impact sample are used in this report: the impact sample for the Atlanta, Grand Rapids, and Riverside education-focused programs (box 2); and the impact sample for all 11 programs with three years of follow-up administrative records and basic demographic baseline data (box 3). The impact sample for education-focused programs in Atlanta, Grand Rapids, and Riverside was used to produce Table 2.2. The impact sample with three years of administrative follow-up data for the 11 programs in the evaluation was used to produce the estimates presented in Tables 5.1 and 5.3 through 5.10. As mentioned previously, this sample is slightly smaller than the full impact sample (box 1) because three years of administrative records are not available for the late cohort in Oklahoma City.

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[5] The survey sample was randomly selected from a larger sample of randomly assigned individuals. In Atlanta, Grand Rapids, and Riverside, certain subgroups were oversampled to produce samples that were large enough for special analyses. In this report, the survey samples in these sites are weighted to replicate the demographic characteristics of their larger sampling frame.

[6] This test was not administered to mothers with children in this age range because they were given an expanded version of the Two-Year Client Survey which included a module intended to gather information about their children's well-being. Retaining the math test would have made the survey too long for this group.

[7] Random assignment in Oklahoma City occurred from September 1991 through May 1993. Three years of administrative records data are available for those randomly assigned from September 1991 through September 1992.

Survey Sample. The sample of individuals for whom Two-Year Client Survey data are available is known as the *survey sample*. Box 4 of Figure A.1 shows that these data are available for 4,833 sample members without a high school diploma or GED at random assignment in all 11 programs.

The survey sample for the Atlanta, Grand Rapids, and Riverside education-focused programs (box 5) comprises 2,923 individuals without a high school diploma or GED at random assignment. This sample was used to produce the two-year impacts on participation in program activities and receipt of educational credentials (presented in Tables 2.3, 2.4, and 2.5).

The TALS document literacy test sample (box 6) is composed of individuals from the education-focused programs in Atlanta, Grand Rapids, and Riverside who had valid baseline and follow-up TALS document literacy test scores (n=2,532). This sample was used to produce the impacts on reading scores (Tables 2.6 and 2.7). As mentioned above, mothers with children from 3 to 5 years old at random assignment were not administered the CASAS math test. Therefore, the CASAS math test sample shown in box 7 (n=1,457) is made up of a small number of sample members with children under 3 years of age at random assignment and sample members with children 6 or older at random assignment. Box 8 shows that 1,326 sample members whose youngest child was 6 or older at random assignment had valid baseline and follow-up reading and math test scores. The TALS document literacy test and CASAS math test sample was used to produce the results shown in Table 2.8.

The samples used in Chapters 3, 4, and 6 were created from different combinations of the samples shown in Figure A.1 and have further restrictions. Chapter 3 focuses on three primary outcomes: GED or high school diploma receipt, reading achievement as measured by the TALS document literacy test, and math achievement as measured by the CASAS math test. Analyses of each of the three outcomes were restricted to survey sample members from education-focused programs in Atlanta, Grand Rapids, and Riverside (box 5) who had participated in a basic education activity during the follow-up period and who were not missing any of the baseline control variables used in the chapter's regression models. The background characteristics presented in Table 3.1 are the baseline control variables used in all the regression models in Chapter 3. Because of these restrictions, the sample sizes used in Chapter 3 are somewhat lower than shown in boxes 5, 6, and 7. The sample size for the analysis of GED or high school diploma receipt is 1,962 rather than 2,923 (box 5); the reading test sample used in Chapter 3 is 1,955 rather than

2,532 (box 6); and the math test sample used in Chapter 3 is 1,139 rather than 1,457 (box 7).[8] These three samples were used to produce the numbers presented in Tables 3.2 through 3.7.

In addition, Chapter 3 investigates the relationship between the characteristics of adult education providers and the payoff in terms of education credential receipt and TALS document literacy test scores to time spent in adult education. For this analysis, the samples discussed above were further restricted to individuals who attended adult education at an education provider that responded to the Adult Education Teacher Survey. Of the 1,962 individuals used in the analysis of GED or high school diploma receipt, 360 met this criterion. Of the 1,954 individuals used in the analysis of reading test scores, 452 individuals met this criterion. These samples were used to produce the results presented in Tables 3.8 and 3.9. The relationship between adult education providers and the payoff in terms of math test scores to time spent in adult education was not analyzed for reasons discussed in Chapter 3.

Chapter 4 investigates the relationship between three measures of educational achievement (GED or high school diploma receipt, reading achievement, and math achievement) and earnings and AFDC outcomes. All the samples used in Chapter 4 have three years of administrative records follow-up data (box 3) and were administered the Two-Year Client Survey (box 4),[9] which results in a total sample size of 4,274. This sample was used to produce the results shown in Tables 4.1 through 4.6. About half of this sample (2,217) had earnings and thus were used in the analysis of earnings per quarter worked presented within Tables 4.2 through 4.5 and 4.7. The sample size for analyses involving TALS document literacy test data is 2,296 (rather than 2,532, as shown in box 6); and for analyses involving CASAS math test data the sample size is 1,323 (rather than 1,457, as shown in box 7). Again, about half of both the reading and the math test samples were used in analyses conducted on only individuals with earnings. TALS document literacy test and CASAS math test samples were used to produce the results shown in Tables 4.6 and 4.7.

Chapter 6 investigates the links between basic education and participation in postsecondary education. Therefore, only those survey sample members who participated in a basic education activity were included in these analysis (n=1,330). This sample size is small for certain analyses that incorporate test score data or POS data or that meet other analytical restrictions. See the text in Chapter 6 for a description of these additional restrictions.

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[8] Note that the CASAS math test sample used in Chapter 3 is unlike the TALS document literacy test and the CASAS math test sample (box 8) in that it is not restricted to sample members whose youngest child was 6 or older at random assignment. Thus, this sample contains individuals who had children under 3 and 6 or older at random assignment.

[9] As mentioned previously, unlike in the majority of this report, sample members in Riverside's education-focused program who had a high school diploma or GED at random assignment but had low test scores or limited English skills were excluded from Chapter 4's analysis because of its focus on the receipt of a high school diploma or GED by the end of two years. For individuals who already had one of these credentials at random assignment such an analysis is fruitless.

Appendix B

Education Institution Scale Creation

Below is an enumeration of items used in creating the scales of adult education provider characteristics presented in Chapter 2, Table 2.1. All the items were taken from the Adult Education Teacher Survey conducted by MDRC as part of the NEWWS Evaluation. This survey is available from MDRC. The letters and numbers before each item refer to its location in the questionnaires.

An education institution's score on the scales presented in this report represents the average of its teachers' scores. The number of teachers surveyed at each education institution is shown in Table 2.1. Thus, this analysis assumes that the average teacher's response to the Adult Education Teacher Survey is representative of the characteristics of the education institution as a whole.

Item responses from the Adult Education Teacher Survey were based on a 7-point metric ranging from low (1) to high (7). The original range of each item below is presented in parentheses. It was sometimes necessary to reverse the direction of the scales for comparability with other measures; in such cases, this is noted below.

Factor analysis and a review of relevant literature were conducted to determine meaningful scale components. Cronbach's alpha calculation—a statistical measure of a scale's reliability—was conducted on each factor-based scale. Coefficient alphas of .70 are generally considered acceptable.[1] One scale's alpha had a value of .65; the remainder of the alphas ranged from .75 to .93.[2]

Items for which respondents indicated "don't know" or "refused" were recoded to a missing value unless otherwise noted. If half or more of the values in a scale were valid responses, missing values were replaced with the mean of the nonmissing values for a scale.

Scale scores were created by summing the value of the responses to items in each scale. For readability, each mean scale score was divided by the number of items summed to approximate the original metric of the items used to construct the scale. Next, variables with scores that indicated high, medium, and low on the scale were created from the scale variables. Average scores from 5 to 7 on scales were coded as high, scores from 1 to 3 were coded as low, and scores greater than 3 but less than 5 were coded as medium.

Items Used in Table 2.1: Characteristics of Major Education Institutions Providing Adult Education to Sample Members in Three Education-Focused Programs

A. Communication Between JOBS Program and Education Institution Staff

Cronbach's alpha = .85

This scale was designed to measure the degree of communication and information-sharing between the Job Opportunities and Basic Skills Training (JOBS) program and the education institutions. The scale was created from the following items:

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[1] See Hatcher, 1994, p. 137.

[2] Similar measures to those discussed here were reported in Hamilton et al., 1997. However, in this report the Cronbach's alphas and frequencies of individual measures vary from those previously presented because of a difference in the unit of observation: the unit of observation in this report is the education institution, whereas previously reported numbers were based on teacher-level data. See Hamilton et al., 1997, pp. 281-292, for a description of measures constructed from the Adult Education Teacher Survey in which individual teachers were the units of observation.

H1. On average, how often do *you* talk to JOBS staff, either by telephone or in person?

a. Daily
b. 2-4 times per week
c. Once per week
d. Once every 2-3 weeks
e. Once a month
f. Once every 2-3 months
g. Less than once every 3 months
h. I do not talk to JOBS staff

H3. How often do you have discussions with a JOBS staff member about *a student*?

a. Daily
b. 2-4 times per week
c. Once per week
d. Once every 2-3 weeks
e. Once a month
f. Once every 2-3 months
g. Less than once every 3 months

H5. How often have you had discussions with JOBS staff about the education of JOBS students in general?

a. Daily
b. 2-4 times per week
c. Once per week
d. Once every 2-3 weeks
e. Once a month
f. Once every 2-3 months
g. Less than once every 3 months

Questions H1, H3, and H5 were converted into 7-point scales with daily interaction coded as 7 and infrequent interaction coded as 1. Those individuals who said that they did not talk to JOBS staff in question H1 were coded as a 1 on the 7-point scale.

H7. In general, how knowledgeable are you about the goals, procedures and rules of the JOBS program? ("Not knowledgeable" to "Very knowledgeable")

Question G6 asks teachers to report after how many days (from "After 1-2 days" to "Not at All") they or someone else at the program would do something to find out what was wrong. As a follow-up to this question, teachers were asked, "What would you or someone at your program do in this situation?" Teachers then were asked to rate the likelihood of notifying the JOBS program as follows:

G7e. Tell the JOBS program ("Would not do this" to "Would do this")

B. Degree to Which Skills Tested in the TALS Document Literacy Test Were Taught in Class

Cronbach's alpha = .80

This scale was created from items 01 to 05 on the survey. Teachers were asked to examine a series of questions that appear on the Test of Applied Literacy Skills (TALS) document literacy test. They were then asked whether the skills and content area covered by the test questions were taught in their class ("No" or "Yes"). The actual test questions are not reported here because they are based on graphs, maps, and charts.

C. Degree to Which Skills Tested in the CASAS Math Test Were Taught in Class

Cronbach's alpha = .85

This scale was created from items 06 to 09 on the survey. Teachers were asked to examine questions that appear on the Comprehensive Adult Student Assessment System (CASAS) math test. They were then asked whether the skills and content area covered by the test questions were taught in their class ("No" or "Yes"). The actual test questions are not reported here because they are based on charts and mathematical computations.

D. Education/Work Link

Cronbach's alpha = .93

This scale indicates the degree to which education providers attempt to integrate work preparation into their educational curriculum. The scale was created from the following items:

F1. Does your class try to prepare students for work in any of the following ways? ("Do not teach" to "Strongly emphasize")

a. Teaching how to read and reply to employment ads and job application forms

b. Practicing how to write a résumé

c. Teaching how to apply math skills to work-related situations

d. Using reading materials about work situations

e. Teaching career awareness

f. Teaching how to do well during an employment interview

g. Teaching important work habits such as punctuality and regular attendance

h. Teaching appropriate dress and grooming skills for work situations

i. Teaching about or making referrals to local vocational training programs

j. Conducting field visits to employers or bringing employers into the class

k. Teaching about job benefits such as unemployment and health insurance

F2. Do you ever refer students to job search programs or activities? ("Never" to "Very often")

E. Degree of Individual Attention

Cronbach's alpha = .75

This scale was created from the following items:

How much are each of the following activities used in your class?

C4a. One-on-one instruction with a teacher or tutor ("Not at all" to "A lot")

C4b. The teacher working with small groups of students ("Not at all" to "A lot")

C4c. Whole-class teaching ("Not at all" to "A lot"; direction of scale reversed)

C9. Do you spend more time developing lesson plans for individual students or for the class as a whole? ("Individual students" to "Class as a whole"; direction of scale reversed)

C10. Is the sequence of activities that students follow the same for all or nearly all students in your class or is this sequence different for all or nearly all students? ("Same for nearly all students" to "Different for all students")

F. Emphasis on Personal Relationships

Cronbach's alpha = .83

This scale measures the degree to which education institutions provide and/or emphasize the importance of personal interaction between students and their teachers, counselors, and fellow classmates. The scale was created from the following items:

D1. With how many students in your class have you had one-to-one conversations about their educational experiences, such as their past educational experiences, their educational goals, or their attendance or progress in your class? ("None" to "All")

D2a. With how many students in your class have you had one-to-one conversations about their personal life, such as their spouses/partners and children or their personal problems? ("None" to "All")

D2b. How important does *your program* think it is for you to develop a counseling relationship with students? ("Not important" to "Very important")

D3. How important do *you* think it is for you to develop a counseling relationship with students? ("Not important" to "Very important")

D4. To what extent do you think students in your class form close friendships and support networks with each other? ("Never" to "Very often")

G. Average Rating of Teaching Materials and Equipment

Cronbach's alpha = .75

This scale indicates the teachers' average rating of materials and equipment provided by their institution. The scale was created from the following items:

How would you rate your programs' resources? ("Poor" to "Excellent")

L24a. The physical plant

L24b. The *availability* of teaching materials (i.e., books, workbooks, tests, etc.)

L24c. The *quality and content* of teaching materials (i.e., books, workbooks, tests, etc.)

L24d. The availability of classroom equipment including computers and software

H. Average Rating of Classroom Morale

Cronbach's alpha = .65

This scale indicates teachers' average job satisfaction and overall staff morale at education providers that served JOBS clients. The scale was created from the following items:

L1. My program is a good place for teachers to work. ("Strongly disagree" to "Strongly agree")

L2. All things considered, how satisfied are you with your current teaching job? ("Very dissatisfied" to "Very satisfied")

L3. How would you describe the morale among the staff who work in your adult education program (i.e., ABE, GED, ESL)? ("Very low" to "Very high")

I. Self-Perceived Teacher Effectiveness

Cronbach's alpha = .86

The scale was created from the following items:

This scale measures how teachers at education providers rated their own teaching effectiveness. The following questions are about your opinions about teaching.

N20. How often are you able to get through to even the most difficult or unmotivated students? ("Not very often" to "Very often")

N22. I am certain I am making a difference in the lives of my students. ("Strongly disagree" to "Strongly agree")

N24. When it comes right down to it, a teacher at my program really can't do much—the students have too many problems in their lives. ("Strongly disagree" to "Strongly agree"; direction of scale reversed)

Appendix C

Supplementary Tables to Chapter 2

Appendix Table C.1

For Sample Members Without a High School Diploma or GED at Random Assignment: Selected Characteristics of Sample Members in Three Education-Focused Programs, by Program

Characteristic	Atlanta	Grand Rapids	Riverside
Demographic Characteristics			
Gender (%)			
Male	3.6	4.3	9.7
Female	96.4	95.7	90.3
Age (%)			
Less than 19	0.0	9.4	1.1
19-24	11.0	36.2	15.9
25-34	52.8	37.5	49.4
35-44	29.4	13.2	26.1
45 and over	6.9	3.7	7.6
Average age (years)	33.0	27.2	32.1
Ethnicity (%)			
White	5.0	47.9	38.7
Black	92.9	37.9	16.3
Hispanic	0.9	12.4	39.7
Other	1.2	1.8	5.2
Family Status			
Marital status (%)			
Never married	61.5	62.9	34.6
Married, living with spouse	1.7	3.8	9.1
Separated	20.3	18.2	32.9
Divorced	14.7	14.1	21.7
Widowed	1.8	1.0	1.8
Number of children (%)			
1	31.8	43.5	35.6
2	30.6	36.5	31.4
3 or more	37.6	20.0	33.0
Average number of children	2.3	1.8	2.1
Age of youngest child (%)			
2 or under	0.5	48.5	7.4
3 to 5	38.9	20.5	49.5
6 or over	60.6	31.0	43.1

(continued)

Appendix Table C.1 (continued)

Characteristic	Atlanta	Grand Rapids	Riverside
Education Status			
Received high school diploma or GED certificate (%)	0.0	0.0	22.6
Average highest grade completed	10.0	9.9	10.0
Highest grade completed			
8 or below	11.5	12.7	18.1
9 or 10	44.1	44.7	26.4
11 or above	44.4	42.6	55.5
Enrolled in education or training in past 12 months (%)	9.9	44.2	15.7
Currently enrolled in education or training (%)	5.4	36.5	10.0
Reading and Math Skills			
TALS document literacy test[a]			
Scored at (%)			
Level 1	30.6	14.8	10.9
Level 2	44.8	41.3	44.2
Level 3	20.6	32.6	32.8
Level 4	4.0	11.3	12.1
Level 5	0.0	0.1	0.0
Level 1 - 2	75.4	56.1	55.2
Level 3 - 5	24.6	43.9	44.9
CASAS math test			
Scored at (%)			
Level 1	31.7	10.8	11.6
Level 2	47.5	42.7	50.4
Level 3	16.5	26.6	22.6
Level 4	4.3	20.0	15.4
Level 1 - 2	79.2	53.5	62.0
Level 3 - 4	20.8	46.5	38.0
Labor Force Status			
Worked full time for 6 months or more for one employer (%)	61.7	50.6	62.5
Any earnings in past 12 months (%)	21.0	36.7	34.0
Currently employed (%)	4.9	9.0	7.4

(continued)

Appendix Table C.1 (continued)

Characteristic	Atlanta	Grand Rapids	Riverside
Public Assistance Status			
Total prior AFDC receipt[b] (%)			
None	0.5	0.1	1.1
Less than 1 year	16.1	18.9	29.8
1 year or more but less than 2 years	6.8	18.2	10.0
2 years or more but less than 5 years	21.4	29.3	26.3
5 years or more but less than 10 years	21.8	17.7	17.3
10 years or more	33.5	15.9	15.5
Raised as a child in a household receiving AFDC (%)	33.7	40.5	22.4
First spell of AFDC receipt[c] (%)	4.6	25.6	21.9
Housing Status			
Current housing status (%)			
Public housing	41.9	2.7	3.2
Subsidized housing	21.1	10.8	7.9
Emergency or temporary housing	1.4	2.0	1.1
None of the above	35.6	84.5	87.8
Sample size	1,519	1,209	3,135

SOURCES: MDRC calculations from information routinely collected by welfare staff, and from TALS document literacy test and CASAS math test data.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen.

Distributions may not add to 100 percent because of rounding.

TALS = Test of Applied Literacy Skills.

CASAS = Comprehensive Adult Student Assessment System.

[a] TALS document literacy test scores for Riverside are based on scores earned on the GAIN Appraisal literacy test and are converted to their TALS equivalent.

[b] This refers to the total number of months accumulated from one or more spells on an individual's own or spouse's case. It does not include AFDC under a parent's name.

[c] This does not mean that such individuals were new to the AFDC rolls, only that this was their first spell on AFDC. This spell, however, may have lasted several years.

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Appendix Table C.2

For Sample Members Without a High School Diploma or GED at Random Assignment: Two-Year Impacts on Participation in Program Activities in Three Education-Focused Programs, by Program

	Participated (%)			Hours of Participation			*Hours of Participation Among Participants*		
Program and Activity	Program Group	Control Group	Difference (Impact)	Program Group	Control Group	Difference (Impact)	*Program Group*	*Control Group*	*Difference (Impact)*
Atlanta									
Any activity	53.0	16.7	36.3 ***	311.6	60.2	251.3 ***	*587.4*	*360.4*	*227.0*
Job search	10.6	3.8	6.7 ***	15.5	3.2	12.3 ***	*146.9*	*83.0*	*63.9*
Education or training activity	46.4	13.1	33.3 ***	296.0	57.1	239.0 ***	*637.5*	*434.7*	*202.8*
Adult education	43.0	9.0	33.9 ***	255.2	29.3	225.9 ***	*594.1*	*325.2*	*268.8*
ABE or GED	42.6	8.5	34.0 ***	244.1	24.2	219.9 ***	*573.6*	*284.4*	*289.3*
ESL	0.2	0.0	0.3	0.9	0.0 [b]	0.9	*428.5*	*0.0*	*428.5*
High school completion[a]	1.1	0.6	0.5	10.2	5.3	4.9	*967.7*	*922.9*	*44.8*
College	0.7	0.0	0.7 **	7.7	0.0 [b]	7.7	*1067.4*	*0.0*	*1067.4*
Vocational training	5.7	5.2	0.5	33.1	27.8	5.2	*583.2*	*538.6*	*44.6*
Work experience or on-the-job training	1.8	1.2	0.6	N/a	N/a		*N/a*	*N/a*	
Adult education *and* job search	4.1	0.9	3.2 ***	23.8	4.9	19.0 **	*577.3*	*545.3*	*32.0*
Adult education *and* college or vocational training	2.7	1.1	1.7 **	38.7	9.5	29.2 **	*1415.6*	*905.4*	*510.2*
Sample size	561	531		561	531		*(varies)*	*(varies)*	

(continued)

Appendix Table C.2 (continued)

	Participated (%)			Hours of Participation			*Hours of Participation Among Participants*		
Program and Activity	Program Group	Control Group	Difference (Impact)	Program Group	Control Group	Difference (Impact)	*Program Group*	*Control Group*	*Difference (Impact)*
Grand Rapids									
Any activity	72.5	41.6	30.8 ***	402.9	221.4	181.4 ***	*556.0*	*532.1*	*23.9*
Job search	20.5	6.0	14.5 ***	26.8	4.8	22.0 ***	*130.7*	*79.7*	*51.0*
Education or training activity	66.8	39.6	27.2 ***	376.1	216.6	159.5 ***	*562.6*	*546.5*	*16.2*
Adult education	58.4	31.9	26.6 ***	271.7	148.0	123.7 ***	*465.1*	*464.6*	*0.4*
ABE or GED	47.7	23.7	24.0 ***	188.0	90.2	97.8 ***	*394.0*	*380.1*	*13.9*
ESL	1.1	1.8	-0.6	6.6	14.5	-7.8	*592.5*	*821.4*	*-228.9*
High school completion[a]	12.0	9.0	3.0	77.0	43.4	33.7 *	*644.0*	*481.4*	*162.6*
College	7.1	3.5	3.6 *	39.3	21.3	18.0	*554.4*	*615.5*	*-61.1*
Vocational training	14.6	8.8	5.7 *	65.1	47.3	17.8	*447.0*	*535.8*	*-88.8*
Work experience or on-the-job training	2.2	1.2	1.1	N/a	N/a		*N/a*	*N/a*	
Adult education *and* job search	14.9	3.7	11.2 ***	88.6	23.3	65.3 ***	*595.3*	*633.8*	*-38.6*
Adult education *and* college or vocational training	13.2	4.1	9.1 ***	116.1	36.9	79.2 ***	*878.9*	*894.9*	*-16.0*
Sample size	233	248		233	248		*(varies)*	*(varies)*	

(continued)

Appendix Table C.2 (continued)

Program and Activity	Participated (%)			Hours of Participation			*Hours of Participation Among Participants*		
	Program Group	Control Group	Difference (Impact)	Program Group	Control Group	Difference (Impact)	*Program Group*	*Control Group*	*Difference (Impact)*
Riverside									
Any activity	65.1	25.2	39.8 ***	294.9	90.1	204.7 ***	*453.1*	*357.0*	*96.1*
Job search	26.2	5.9	20.3 ***	31.5	6.8	24.7 ***	*119.8*	*114.4*	*5.5*
Education or training activity	56.8	21.4	35.3 ***	263.4	83.4	180.1 ***	*464.1*	*388.7*	*75.4*
Adult education	49.3	11.5	37.8 ***	205.9	27.7	178.2 ***	*417.7*	*241.4*	*176.3*
ABE or GED	38.1	6.3	31.8 ***	136.1	11.1	125.0 ***	*357.1*	*176.6*	*180.5*
ESL	9.1	2.7	6.4 ***	54.0	7.7	46.4 ***	*595.6*	*284.2*	*311.4*
High school completion[a]	5.0	3.1	1.9 *	15.8	8.9	6.8	*317.1*	*289.7*	*27.5*
College	5.7	4.8	0.9	22.4	21.0	1.4	*393.3*	*441.2*	*-47.8*
Vocational training	8.3	8.1	0.1	35.1	34.6	0.4	*424.8*	*425.9*	*-1.1*
Work experience or on-the-job training	1.8	1.0	0.8	N/a	N/a		*N/a*	*N/a*	
Adult education *and* job search	16.0	1.4	14.6 ***	89.2	5.7	83.4 ***	*556.4*	*408.5*	*147.9*
Adult education *and* college or vocational training	5.9	2.1	3.8 ***	44.6	14.0	30.6 ***	*758.5*	*663.8*	*94.7*
Sample size	621	729		621	729		*(varies)*	*(varies)*	

(continued)

Appendix Table C.2 (continued)

SOURCE: MDRC calculations from the JOBS Two-Year Client Survey.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

Survey respondents were interviewed between month 25 and month 31, counting the month in which random assignment occurred as month 1.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen.

Estimates were regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members.

Sample sizes for individual measures vary because of missing values.

Numbers may not add up to 100 percent because of rounding.

A two-tailed t-test was applied to differences between outcomes for the program and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

Italics are used to signal average outcomes and differences that were calculated only for participants. Unlike the full-sample program and control groups, these program and control groups may differ from each other in average background characteristics. Such differences could have influenced the types of employment-related activities people in the two groups attended or their length of stay. If so, the program-control differences might understate or overstate the effects of the programs. Because these impact estimates are less reliable than those based on the full sample, statistical significance tests of these results were not conducted.

N/a = not available or applicable.

ABE = Adult Basic Education.

ESL = English as a Second Language.

[a] This may include regular high school classes.

[b] The adjusted control mean is actually slightly negative.

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Appendix Table C.3

For Sample Members Without a High School Diploma or GED at Random Assignment: Impacts on TALS Document Literacy Test Scores in Three Education-Focused Programs, by Program

Program and Measure	Program Group	Control Group	Difference (Impact)	Effect Size[a]
Atlanta				
Average TALS document literacy test score	232	230	2.1	0.04
Scored at (%)				
Level 1	37.1	38.3	-1.2	-0.03
Level 2	44.0	45.3	-1.3	-0.03
Level 3	16.9	13.5	3.4	0.08
Level 4	2.0	2.6	-0.6	-0.02
Level 5	0.0	0.3	-0.3	-0.09
Sample size	538	507		
Grand Rapids				
Average TALS document literacy test score	260	261	-1.6	-0.03
Scored at (%)				
Level 1	20.0	16.6	3.4	0.08
Level 2	40.5	45.9	-5.3	-0.11
Level 3	33.3	26.9	6.5 *	0.15
Level 4	6.1	10.6	-4.5 *	-0.18
Level 5	0.0	0.0	0.0	0.00
Sample size	224	240		
Riverside				
Average TALS document literacy test score	260	260	-0.7	-0.01
Scored at (%)				
Level 1	20.7	18.1	2.5	0.06
Level 2	39.2	41.8	-2.6	-0.05
Level 3	30.8	32.9	-2.1	-0.05
Level 4	9.3	7.1	2.2	0.09
Level 5	0.0	0.0	0.0	0.00
Sample size	468	555		

(continued)

Appendix Table C.3 (continued)

SOURCE: MDRC calculations from TALS document literacy test data.

NOTES: About a quarter of the sample in Riverside's education-focused program did have a high school diploma or GED, but they scored low on the math or reading portion of the appraisal test or had limited English, and thus were determined to be in need of basic education.

To compensate for differences in the proportion of subgroup members chosen to be surveyed, respondents were weighted by the inverse of the probability of their being chosen.

Estimates were regression-adjusted using ordinary least squares, controlling for pre-random assignment characteristics of sample members.

Rounding may cause slight discrepancies in calculating sums and differences.

A two-tailed t-test was applied to differences between program and control groups. Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; and *** = 1 percent.

TALS = Test of Applied Literacy Skills.

[a]The effect size equals the difference (impact) divided by the standard deviation of the outcome measure for the full sample control group.

Appendix D

Description and Interpretation of the TALS Document Literacy Test and the CASAS Math Test

The Test of Applied Literacy Skills (TALS) document literacy test attempts to measure individuals' skills in understanding and using printed and written information needed to manage everyday life.[1] This definition of literacy implies a set of information-processing skills that go beyond the decoding and comprehension of texts used in some other standardized tests. The test was designed to measure skills needed outside purely academic settings, that is, skills required for effective functioning in the workplace and in a complex technological society.

The TALS document literacy test, as its name implies, was designed to measure skills needed to work with documents. This test requires readers to use schedules, charts, graphs, maps, and forms. Performance on this test reflects knowledge associated with finding and transferring information from one document to another. For example, one test item asks the respondent to read a line graph of car prices and to determine when the cost of a car was at its highest.

The test is not in multiple-choice format; it requires the test-taker to write in the answer. In scoring, the number of correct items is converted into a scale score that ranges from zero to 500. The Educational Testing Service of Princeton, New Jersey, intended that TALS document literacy scale scores be interpreted as reflecting five literacy levels. It is not possible or appropriate to translate these test scores into grade-level equivalents.

A description of the literacy skills associated with each of the five literacy levels is provided in Appendix Table D.1. Individuals estimated to be performing at the indicated level can complete the specified tasks with a high degree of consistency—80 percent of the time. Consider as an example a person who is performing at the 250 level on the document scale. This individual can be expected to perform tasks that are rated at this level correctly 8 out of 10 times, that is, with an 80 percent probability of performing them correctly. This same individual would be expected to respond correctly to an item at the 300 level 40 percent of the time and could perform tasks at the 200 level with more than a 90 percent probability of responding correctly.[2]

The TALS document literacy test contains 26 items, with levels of difficulty ranging from 179 to 408 on the test scale. The difficulty of each question reflects the number of categories or features of information in the question, the number of distractors or plausible answers, the degree to which the test question is obviously connected with the information stated in the document, and the complexity of the document. As illustrated by Appendix Table D.1, the easiest questions are those that require individuals to directly match a single piece of information in the question and text (for example, to enter the account information on a savings withdrawal form). More difficult tasks require respondents to engage in a series of matches or to compare and contrast information in adjacent parts of the document (for example, to use a map and its legend to locate a building).

The TALS test was chosen to measure achievement outcomes in the NEWWS Evaluation after a review of 10 leading achievement tests and after consultation with experts. It was chosen with the aim of measuring basic literacy skills related to employment, since employment was the ultimate goal of the JOBS program. The emphasis of the TALS test on life skills materials and competencies was considered more appropriate than an academic test for measuring the skills of

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[1] This discussion first appeared in Martinson and Friedlander, 1994.

[2] Kirsch and Jungeblut, 1992, pp. 85-86.

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Appendix Table D.1

Description of Literacy Levels on TALS Document Literacy Test

Level and Score Range	Description of Tasks Required by Test Items[a]	Examples of Test Tasks[b]
Level 1 0-225	Tasks at this level are the least demanding. They typically require the reader to make a literal match between a single piece of information stated in a question and information provided in text or to enter information from personal knowledge.	Enter account information on a bank's savings withdrawal form. (200)
Level 2 226-275	Tasks at this level also involve a single piece of information; however, several plausible choices for matching that are not correct may be presented. Also, the match may not be literal and may require drawing inferences from the text.	Using a line graph of car prices, determine when the price of a particular car peaked. (268)
Level 3 276-325	Tasks require the matching of more than two pieces of information. The information is presented in more complex displays and is more subtly differentiated.	Using a hospital campus map and its legend, identify a building that houses a specified medical department. (288)
Level 4 326-375	Tasks require multiple-feature matching and integrating information from complex displays; however, the degree to which the reader must draw inferences is increased from the previous level.	Circle information relating to rates on a page from a telephone book. (358)
Level 5 376-500	Tasks require a high degree of inferential reasoning and integrating information from several sources. Tasks at this level require the ability to process information with a high degree of consistency using several documents.	Interpolate information on a line graph to determine profits in a specified year. (408)

SOURCES: Descriptions of literacy levels are adapted from Kirsch, Jungeblut, and Campbell, 1992; the examples of test tasks are from Educational Testing Service, 1992.

NOTES: This table first appeared as Table B.1 in Hamilton and Brock, 1994.

[a] A person at a given literacy level has an 80 percent or greater chance of performing tasks of the level's type correctly.

[b] The difficulty of actual test items that require these operations is shown in parentheses.

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Appendix Table D.2

CASAS Math Test Score Scale Characteristics and Interpretation

Score Range (Level)	Description of Score Range
Below 200 (Level 1)	Adults in this score range have difficulty with the basic computational skills necessary to function in an employment setting and in the community.
200-214 (Level 2)	Adults in this score range lack the ability to perform functional math tasks required for most jobs.
215-224 (Level 3)	Adults in this score range are able to perform some basic computational skills, although they are only marginally able to perform math tasks in the workplace. They are functioning below a high school level.
225 and above (Level 4)	Adults in this score range can function at a high school entry level in basic math and, if they do not have a high school diploma, can profit from instruction in General Educational Development (GED) classes and have a high probability of passing the GED test in a short time.

SOURCE: Comprehensive Adult Student Assessment System (CASAS), 1990.

JOBS registrants, given the goals of the JOBS program. The TALS test was also selected for its high internal reliability and its use in national studies, which allows comparisons of the literacy levels of the JOBS population with those of other groups. It is important to understand that this test was not used by the county education programs in this study to gauge student progress. Thus, the schools were not teaching the specific items in this test.

Appendix E

Private Opinion Survey Scales and Subgroups

Below is an enumeration of items used in the creation of subgroups and scales taken from the Private Opinion Survey (POS) conducted by MDRC as part of the NEWWS Evaluation. This survey is available from MDRC. The letters and numbers before each item refer to its location in the questionnaires.

Item responses from the POS were most often based on a 4-point metric, although some items were dichotomous. The original metric and range for each item below are presented in parentheses. For comparability within and across scales, variables were recoded in two ways. First, because items based on a 4-point metric and dichotomous variables were both used to create the scales discussed below, dichotomous variables were recoded to match their counterpart values on the 4-point scales. Second, in many cases the direction of the scales was reversed so that higher scores indicated more barriers to participation. When an item was recoded or the original direction was reversed, it is noted below.

Factor analysis was conducted to determine meaningful scale components. Cronbach's alpha calculation—a statistical measure of a scale's reliability—was conducted on each factor-based scale. Coefficient alphas of .70 are generally considered acceptable.[1] One scale presented below has an alpha of .66,[2] and the rest range from .70 to .89.

Items for which respondents indicated "don't know" or "refused" were recoded to a missing value. If half or more of the values in a scale were valid responses, missing values were replaced with the mean of the nonmissing values for a scale.

Scale scores were created by summing the value of the responses to items in each scale. For readability, each mean scale score was divided by the number of items summed to approximate the original metric of the items used to construct the scale. Next, subgroups were created from the scale variables using the methods discussed below.

A. Reported Barriers to Participation Subgroups

This set of subgroups was created from a combination of two scales: (1) a scale indicating the degree to which respondents perceived family and personal problems as a barrier to participation in school or to work and (2) a scale indicating the degree to which respondents preferred to stay at home with their family rather than go to school or work. It is intended to measure the effects of having multiple barriers to participation as well as to isolate the effects of two types of barriers to participation.

A.1 Family or Personal Problems Scale

Cronbach's alpha = .81

The scale was created from the following items:

22. My family is having so many problems that I cannot go to a school or training program right now. (4-point scale: "Agree a lot" to "Disagree a lot"; direction of scale reversed)

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[1] See Hatcher, 1994, p. 137.

[2] This scale was used only to control for selection bias in multiple regressions.

30. My family is having so many problems that I cannot work at a part-time or full-time job right now. (4-point scale: "Agree a lot" to "Disagree a lot"; direction of scale reversed)

I *cannot* go to school or a job training program right now because I . . .

45. Have a health or emotional problem. (4-point scale: "Agree a lot" to "Disagree a lot"; direction of scale reversed)

46. Have a child or family member with a health or emotional problem. (4-point scale: "Agree a lot" to "Disagree a lot"; direction of scale reversed)

49. Already have too much to do during the day. (4-point scale: "Agree a lot" to "Disagree a lot"; direction of scale reversed)

A.2 Parental Concerns

Cronbach's alpha = .70

The scale was created from the following items:

1. If you had a choice, which would you prefer, going to school to study basic reading and math *or* staying home to take care of your family? (Dichotomous variable: "Reading/math" or "Staying home")

25. Right now I'd prefer *not* to work so I can take care of my family full-time. (4-point scale: "Agree a lot" to "Disagree a lot"; direction of scale reversed)

40. I do *not* want a job because I would miss my children too much. (4-point scale: "Agree a lot" to "Disagree a lot"; direction of scale reversed)

47. I *cannot* go to a school or job training program right now because I am afraid to leave my children in day care or with a baby sitter. (4-point scale: "Agree a lot" to "Disagree a lot"; direction of scale reversed)

The dichotomous variable in the Parental Concerns scale was recoded so that the response "reading/math" was equivalent to a "disagree" response and "staying home" was equivalent to an "agree" response on the two 4-point scale variables that are included in this measure.

Both the Family or Personal Problems scale and the Parental Concerns scale were divided into "high" and "low" barriers subgroups using the following method. As indicated above, the 4-point scale variables were reversed so that 1 equals "disagree a lot" and 4 equals "agree a lot." If a sample member's average response across all measures was less than 2.5, then the respondent was classified as low on this scale. If a sample member's average response across all measures in the scale was 2.5 or greater, then the respondent was classified as high on this scale. This effectively divides all sample members between those who, on average, said they disagreed or disagreed a lot to the questions in the scale and those who agreed or agreed a lot to the questions in the scale.

If sample members scored high on both the Family or Personal Problems scale and the Parental Concerns scale, then they were classified as having many family or personal problems *and* parental concerns. If sample members scored high on just the Family or Personal Problems scale but not on the Parental Concerns scale, then they were classified as having many family or personal problems. Conversely, if sample members scored high on just the Parental Concerns scale but not on the Family or Personal Problems scale, they were classified as having many parental concerns. Finally, if sample members scored low on both scales, then they were classified as having neither barrier.

B. Preference for School Scale and Subgroups

Cronbach's alpha = .71

The scale used to create the Preference for School subgroups is intended to measure sample members' motivation to participate in educational activities. It was created from the following items:

6. During the past year, have you told anyone that you wanted to be in a school or training program? (Dichotomous variable: "Yes" or "No")

7. Do you plan to be in a school or training program in the next few months? (Dichotomous variable: "Yes" or "No")

What's your opinion?

26. I like going to school. (4-point scale: "Agree a lot" to "Disagree a lot"; direction of scale reversed)

35. Right now, I'd really like to be going to school to improve my reading and math skills. (4-point scale: "Agree a lot" to "Disagree a lot"; direction of scale reversed)

The dichotomous variables in the Preference for School scale were recoded so that "yes" was equivalent to "agree" and "no" was equivalent to "disagree" on the two 4-point scale variables the make up this measure.

The Preference for School scale was used to create subgroups using the same method as described above for the Family or Personal Problems scale and the Parental Concerns scale. That is, if, on average, sample members agreed or agreed a lot with the questions in the scale, they were placed in the "like and/or plan to attend" school subgroup; and if, on average, sample members disagreed or disagreed a lot with the questions in the scale, they were placed in the "do not like and/or do not plan to attend" school subgroup.

C. Depressive Symptoms Scale and Subgroups

Cronbach's alpha = .89

This scale was created to indicate the degree to which respondents reported having symptoms of depression. It was created from the following items:

During the past week . . .

50. I felt sad. (4-point scale: "Rarely" to "Most or all days")

51. I felt depressed. (4-point scale: "Rarely" to "Most or all days")

52. I felt that I could not shake off the blues, even with the help of family and friends. (4-point scale: "Rarely" to "Most or all days")

53. I felt lonely. (4-point scale: "Rarely" to "Most or all days")

This scale was used to define subgroups as follows. If a respondent's average score across these four questions was 2 or less, then she was classified as having few symptoms. If her average score was greater than 2 but less than or equal to 3, then she was classified as having a moderate number of symptoms. Finally, if her average score was greater than 3, she was coded as having many symptoms on this scale.

D. Mastery Scale

Cronbach's alpha = .66

This scale was used not to create subgroups but rather to control for selection bias in Chapters 3 and 4 of this report. The scale is a modified version of the Pearlin Mastery Scale,[3] which is intended to measure the degree to which a person feels in control of the direction of his or her life, as opposed to feeling that external factors have a dominant influence. It contains three highly intercorrelated items taken directly from the Pearlin Mastery Scale (questions 27, 34, 42) and two other items which improved the scale's overall reliability (questions 29, 33). These questions cohere because the items taken from the Pearlin scale address how efficacious respondents feel and the additional questions address respondents' feelings about lacking control over life events.

27. I have little control over the things that happen to me. (4-point scale: "Agree a lot" to "Disagree a lot"; direction of scale reversed)

29. I often feel angry that people like me never get a fair chance to succeed. (4-point scale: "Agree a lot" to "Disagree a lot"; direction of scale reversed)

33. It's unfair to make people on welfare get a job if they don't want to. (4-point scale: "Agree a lot" to "Disagree a lot"; direction of scale reversed)

34. Sometimes I feel that I'm being pushed around in life. (4-point scale: "Agree a lot" to "Disagree a lot"; direction of scale reversed)

42. There is little that I can do to change many of the important things in my life. (4-point scale: "Agree a lot" to "Disagree a lot"; direction of scale reversed)

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[3] Pearlin et al., 1981.

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